THE MEDICAL JOURNAL OF AUSTRALIA

VOL. I.—18TH YEAR. SYDNEY, SATURDAY, JANUARY 31, 1931. No. 5.

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THE STUDY OF MEDICINE.[1]

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IT is my first and pleasing duty to say how much I appreciate the signal honour that has been done me in my selection to be the first occupant of the Bosch Chair of Medicine. I am fully sensible, not only of the great compliment which has been paid me, but also of the responsibility which now devolves upon me to uphold the honour and dignity of the Chair and of the Medical School of the University of Sydney. I do not know which I feel most, nor is my responsibility mitigated by the fact that it falls to me to maintain and to develop to the best of my ability the high traditions set by my predecessor in the Chair of Medicine, Professor Mills. Professor Mills's genial personality, together with his originality as a teacher, has won for him the affection and esteem of all who have been privileged to come into contact with him, and I trust that even in his retirement he may find time to give us the benefit of his wisdom and experience in the tasks that lie before us. May he thus preserve a link with the University and the hospital, both of which he has served with honour and distinction.

In my short acquaintance with Professor Mills I have found him to be one of those individuals endowed by Nature with that insatiable curiosity which drives a man to be continually endeavouring to probe the underlying causes of phenomena. Since we are about to celebrate the two-thousandth anniversary of Virgil's birth, we may perhaps with propriety apply to him that felicitous line in the Georgics:

Felix qui potuit rerum cognoscere causas.

This may well form the text of the present address, as it admirably expresses the spirit of science, which finds its chief delight in interrogating

[1] Read at the first annual reunion of the Royal Prince Alfred Hospital Residents' and Ex-Residents' Association, October 7 to 11, 1930.

Nature and searching out the causes of things. Whereas the poet sang of flocks and fields, it is my present purpose to speak of medicine, which, however, has this in common with agriculture: it is at once a science and an art, it is concerned with the preservation and bringing forth of more abundant life and it has its origin in the needs of practical life.

From the earliest times suffering and the feelings of sympathy evoked by the sight of suffering have caused men to seek means for its alleviation and to acquire skill in the use of remedies. This in turn has led to the study of disease on its own account as well as from the practical necessity of determining those conditions in which remedial measures might be appropriately applied. Knowledge, skill and sympathy have, therefore, at all times been the essential attributes of the physician. Corresponding to and reflecting these various attributes, medicine may be envisaged from three points of view, intellectual, practical and emotional, the intellectual aspect being represented by medical science, the practical and emotional sides together constituting the art of medicine.

The subject I wish to discuss is the method and spirit of science and of art and their applications in research, teaching and the practice of medicine.

It is first necessary to inquire what is meant by scientific method—this method which has given us such power over Nature, a method, also, which, incidentally, received its first clear expression in writings of the father of medicine.

Briefly, it consists of four links in a never-ending chain: First, observation—noting the facts and collecting the data of experience; then induction—the framing of a tentative hypothesis to explain the facts; thirdly, deduction of the consequences of this hypothesis by mathematical or other form of logical reasoning; finally, verification of these consequences by further observation or by experiment. If the results of experiment and observation are not in accord with the hypothesis, a fresh hypothesis must be framed until one is found which accounts for both the primary data and the further facts brought out by observation and experiment. If, on the other hand, the hypothesis is confirmed, it comes to rank as a theory. Such a theory is not, however, to be looked upon as immutable or as the only possible explanation or even as necessarily representing the absolute truth. It is rather to be regarded as an affair of probability and its validity is to be gauged by its usefulness in explaining the facts for the time being. If it has a very high degree of probability, it may finally be elevated to the status of a natural law, that is, a concept or formula expressing the relationship between phenomena.

It is to be noted that there is no essential difference in logical procedure between the observational and the experimental methods. Both begin with observation, pass through the stages of induction and deduction and end with verification by reference to the facts of experience. The difference between the two depends solely upon whether the observer is able or unable to control experience.

For obvious reasons the experimental method has distinct limitations in its application to the human subject. Nevertheless, observationists like Hippocrates or Sydenham and experimentalists such as Galen and Harvey have equal claim to be regarded as scientific. Observation as a rule takes longer to obtain its data and may depend to a certain extent upon chance, but experiment, while swifter and more direct in its interrogation of Nature, is frequently impossible of application. The ancient aphorism of Hippocrates is apposite: "The art is long, life is short, the occasion fleeting, experiment dangerous, judgement difficult." Where the experimental method can be employed, however, it proves to be an instrument of peculiar power. It has as its aim not merely the observation and prediction of the course of events, but the actual mastery of Nature. Hence we find that in this modern experimental era the emphasis in medical science is upon the discovery of the ætiology, pathogenesis, prevention and definitive cure of disease rather than upon diagnosis, prognosis and expectant methods in treatment.

It must not be supposed that one has merely to follow in a formal fashion the various steps in scientific procedure in order to be a successful investigator. This will be evident if we reflect upon some of the mental qualities which must be brought to bear at each stage.

Observation is by no means a passive process. It depends upon interest as well as a certain alertness in recognizing the unfamiliar. Chance may play a part, but of still greater importance is a prepared mind.

Induction is not the mechanical and indiscriminating affair Bacon is reputed to have supposed it to be. To classify the data of experience, to perceive their mutual interrelationships and to conceive a hypothesis in which these interrelationships are expressed, require both insight and imagination.

Deduction, again, necessitates logical and often mathematical power, while experiment calls for skill, patience, perseverance and ingenuity.

It is interesting to trace some of the vagaries of scientific method at each step of the process. The history of medicine provides many examples.

The foundation upon which all science is built, is an appeal to the facts of experience, hence science is in essence empirical and empiricism is the first step in scientific method. But mere facts, isolated and unrelated, do not constitute a science, although they form the raw materials out of which science is built up. It is only when facts have been united by theory that they become embodied in science, whose aim is the formulation of natural laws. Empiricism which stops at brute facts and does not bring imagination and reason to bear, is sterile and lacking in the power of growth and development. Hence the empiric is apt to be ignorant and dependent upon mere rule of thumb. On the other hand, the school of empirics in ancient times did

useful service in checking fruitless speculation and recalling men to the observation of Nature.

The vagaries of induction often arise from the very fact that there is a natural tendency of the mind to form inductions. Hence the untrained observer commonly fails to distinguish between observation and inference. The inordinate and undisciplined use of the imagination may likewise constitute a danger—witness the exuberant ratiocinations and baseless speculations of the dogmatists, systematists and rationalists, both ancient and modern. These, starting often from the most inadequate data or from some *a priori* consideration regarding the nature of man or of disease, pass at one leap to the wildest generalizations; or they permit speculation to follow speculation without returning at each step to the facts of experience—that "rubbing and grinding" at Nature which Hippocrates insisted upon. The outcome of all this is to arrive at conceptions frequently far removed from reality or to succeed in creating nothing but a mere tissue of verbiage. Into this category come the Methodists of old with their syllogisms of *strictum* and *laxum*, paralleled at a later date by the theories of Hoffmann and Brown, the more extreme and unenlightened representatives of the iatro-mechanical, iatro-chemical and vitalist schools, and, in more recent times, the Christian Scientists and, if one may be permitted to say so, the exponents of psycho-analytic theory.

Deduction may, of course, fail through defective logical or mathematical reasoning, but more often because of insufficient data or lack of adequate controls. If we wish to inquire whether a certain serum cures pneumonia, whether fair-haired people are more susceptible to pulmonary tuberculosis than dark-haired ones, or what is the prognosis in Bright's disease, we cannot depend upon the vague impressions of clinical experience, useful and suggestive as these may be. We have to compare a sufficiently large number of individuals who agree as far as possible in all relevant respects, except with regard to the variables we are introducing, and to submit the results to statistical analysis in order to discover the exact degree of correlation between the alleged relata. Too often, in endeavouring to form an opinion as to the value of a remedy, the physician, in his anxiety to do the best for his patient, neglects to control his experience by studying what happens when the remedy is withheld. Moreover, the doctor, whose habitual attitude is one of optimism, is particularly prone to indulge in that kind of "wishful thinking" whereby he is apt to see only those results which are encouraging.

Experiment may prove fallacious unless designed and carried out in such a way as really to test the point at issue. A more culpable error is to record only the successful experiments and suppress those which do not appear to yield results in accord with preconceived hypothesis.

A notable feature in the development of science, although not exactly a part of scientific method, is the tendency to specialization. This arises partly from the practical necessity for concentrating upon limited problems and partly on account of the vast accumulation of knowledge, which makes it impossible for any single individual to master more than a relatively limited field. The inevitable result of this has been to foster a narrowness of outlook among investigators; but synthesis should be complementary to analysis and, considering the accessibility of the details of knowledge and the advances made in theory, the modern thinker is in many respects at a distinct advantage as compared with his forbears in having at his disposal the means of acquiring a grasp of general principles and the attainment of a breadth of culture without prejudice to special competence in some particular sphere. The ancient philosophers were undaunted in their attempt to survey the whole field of Nature and of knowledge, probably because they had little inkling of the complexity of their problems. In this respect they present a contrast with the more modest attitude of the modern scientist. This leads us to consider next the spirit of science.

Science is a rational discipline. It is marked by a slow, humble, cautious and unbiased search for the truth, an unconditional reverence for facts and fidelity in recording them, whether they be conformable to the observer's predilections or otherwise. Its attitude is detached and impersonal, resembling that of the judge rather than that of the barrister, ever ready to suspend judgement in the presence of inadequate evidence. Born of necessity and the natural inquisitiveness of man, it employs its skill in experiment rather than in subtle dialectics; animated by the free spirit of inquiry and purged of what Bacon called the idols of the tribe, the den, the market and the theatre, it knows no authority but facts. Utterly foreign to it is the spirit of commercialism, with its desire for advertisement and personal gain. Hence its loathing and contempt for quackery, whether it be a question of the empirical quack, such as the vendor of secret remedies, or the rational quack, such as the nature healer, the osteopath or the chiropractic.

We may contrast the method and spirit of science with that of art. I take it that the term art, as applied to medicine, means the practical application of a science: skill in translating known principles into action. It therefore refers to practice rather than to theory, to doing rather than to knowing.

An art like medicine is, however, more than a craft. The art of the architect or of the engineer differs from the craft of the carpenter or of the plumber in that it is founded upon fundamental principles, for example, upon physical science and mathematics. Hence, it is not dependent upon rule of thumb, but possesses the power of growth, the capacity to meet new situations or to assimilate new knowledge. Similarly, the art of medicine is based in the first instance upon the ancillary sciences of pathology, physiology, pharmacology and anatomy, while these in turn rest upon the fundamental sciences of physics, chemistry and biology. Neglect of these principles in practice tends to

degrade the art into a mere craft, while failure to apply them in education is both short-sighted and sterilizing.

There is, however, a larger sense in which the term art is used in relation to medicine, a sense which includes the æsthetic factor. It means not only skill in the application of knowledge, but skill in the appreciation of the subjective. This calls for special qualities of sympathy and insight which make the practice of the art an intensely personal affair, dependent upon feeling as well as intellect, thus introducing an element foreign to the cold and impersonal quality of the characteristic scientific mood.

In a many-sided profession such as ours there is room for employment of a great variety of talents and men will find their niche according to their natural aptitudes, be they men of thought, of action or of feeling. Medicine has need of them all.

It is now necessary to inquire how the aforegoing principles may be applied first to research.

While every true scientific investigator should be an artist, it is hardly necessary to say that in research the method and spirit of science are paramount. Medical research is no exception to this rule, and it is by the application of the same method and spirit which have proved so useful in other fields that we may hope to advance the science of medicine. Medicine is a branch of applied biology, having the same fundamental concepts and methods as those of biology. We ought, therefore, to expect of medical research the same standards of scientific accuracy and criticism as those which obtain in the fundamental science itself.

Although medicine is a derivative science, it has grown to such proportions that subdivision and specialization have become both necessary and inevitable; but we may well pause to ask ourselves whether the manner of its growth has not led to the development of conditions ultimately inimical to the advance of medical science. Medicine, one and undivided in the Hippocratic era, is now studied in a number of separate departments and laboratories set aside for special purposes. Some of the subjects represented by these departments have grown into independent sciences, with aims of their own quite apart from any bearing they may have upon medicine. The result is that the essential unity of medical science which is concerned with the study of disease is lost sight of, while medicine, the foster-mother of the sciences, is rather in the position of the old woman who lived in a shoe, so that a little salutary discipline may be necessary in order to restore order and unity in the household. The divisions which exist within the domain of medicine are for the most part arbitrary and provisional, but this in itself would not necessarily constitute an evil, were it not for the fact that the independent development and geographical separation of the various departments has led to the deprivation of essential contacts, to the piling up of uncorrelated observations and to a limitation of the freedom of workers to pursue their problems whithersoever they may lead. Most deplorable of all is the divorce between the clinical and the laboratory study of pathology and therapeutics. Already we find signs that these unnatural barriers are being broken down. Physiologists, chemists and pathologists are invading the clinics, while physicians and surgeons are entering the laboratories, and a host of hybrid subjects, such as clinical or applied physiology, clinical pathology, surgical pathology, clinical bacteriology, clinical biochemistry and clinical therapeutics have made their appearance. Surely the time has come to recognize a tendency which has already established itself and to integrate the forces of research in one institute of medical science, where each worker would be free to follow up his investigations in whatever direction they may naturally lead, emancipated from geographical and intellectual isolation. An opportunity of doing so is afforded by the recent gift of the Rockefeller Foundation to the University of Sydney for the erection of a new medical school and research laboratories in close proximity to the Royal Prince Alfred Hospital.

If we wish to secure the advantages of division of labour and specialized knowledge without the corresponding disadvantages, we must first attempt to find a logical basis for both subdivision and synthesis, and this can only be done by reference to first principles.

For our present purpose we may consider that there are three fundamental sciences: (i) Mathematics, (ii) physical science and (iii) biology, to which some would add a fourth—psychology. The physical sciences, being more abstract (but not more fundamental) than biology, can serve biological investigation, while mathematics, the most abstract of all, can be applied to both the others. Following, with slight modifications, the scheme proposed by Arthur Thomson, we may further subdivide biology into four subsciences, bearing in mind that this subdivision is purely a matter of technical convenience: (*a*) morphology: the study of structure and form, the static relation; (*b*) physiology (and psychology): the study of function, the dynamic relation; (*c*) genetics: the study of growth and development, the historical relation; (*d*) ætiology: the study of the conditions which determine structure, function and development.

Medical science, which we may define as that branch of biology which deals with disease in all its manifestations and relations, may be subdivided in a similar fashon, each subdivision having its correlative in the study of the normal: (*a*) morphology: pathological anatomy and histology; (*b*) physiology (and psychology): pathological physiology, pharmacology and therapeutics (pathological psychology); (*c*) genetics: the study of growth, development and inheritance in relation to disease; also the genetic aspect of disease (nosography); (*d*) ætiology: the study of all the conditions and outward circumstances conducive to the production of disease, for example: parasitology (including medical bacteriology, protozoology, entomology,

helminthology, epidemiology, œcology of disease *et cetera*. Hygiene is related to ætiology as therapeutics is to pathological physiology. Needless to say, there is no sharp line of demarcation between these branches of medical science, as they are all interdependent.

Confining ourselves to the laboratory side of research and neglecting some minor details, the above classification of the fundamental sciences and of the branches of medical science forms a basis of differentiation not merely according to point of view, but approximately according to the types of technique employed, namely, (i) statistical methods, (ii) chemical and physical methods, (iii) *post mortem* and histological technique, medical artistry, (iv) operative, kymographic and other dynamic methods employed in experiments upon the living subject, (v) microbiological technique. It is suggested that instead of having a number of comparable investigations going on in separate, self-contained departments, each having several techniques, it would be an advantage to have the actual laboratory plant of a medical research institute centralized according to the main techniques, while the division of labour and charge of these techniques would, according to the above scheme, run approximately parallel. Each division would constitute a definite charge provided with a staff of expert technicians, but workers would pass freely from one division to another, as required according to the technical character of their research.

Fashions in research are constantly changing, but some such general arrangement as the above would provide the maximum elasticity, economy and efficiency, while allowing scope for the development of individuality as well as for that kind of team work which depends upon real leadership and spontaneous collaboration.

So much for subdivision; but what of the co-ordination of these various activities?

Mere propinquity should act as a uniting principle, but experience shows that more is necessary. I conceive it to be one of the essential functions of the new whole-time chairs of medicine, whose occupants are required to have been through the mill both in the laboratory sciences and in the clinics, to restore and preserve continuity in the study of disease in its clinical and laboratory aspects. So far, this continuity has established itself chiefly in relation to chemical and some graphic methods, but this is partly because of the convenience of bringing these methods into the clinics and partly a question of vogue. Medicine, on the laboratory side, contributes no specific techniques, but it must have at its disposal every one of those we have described. Hence, in order to discharge its functions, medicine must ultimately have either a self-contained department of its own in which all techniques are represented, or it must act as the unifying factor in a more comprehensive scheme. In the latter each special section would be free to pursue its problems along the most academic lines, but each would also be orientated in relation to medicine, with an applied side connected with the practical activities of hospital routine or public health work. The hospital connexion, by providing material, should act as a stimulus to research and should not be regarded as over-emphasizing the human aspect of pathology, the interest and importance of which require no special stress. On the one hand the clinics determine and define medical problems which must be worked out in all their details and bearings and by every available means in the laboratory; conversely, the results of investigation, academic or otherwise, in the fundamental and ancillary sciences must be brought to bear upon the clinics. In order to bring about this free interchange of ideas, medicine must be in a position to make the necessary contacts and this can best be done by itself assuming the rôle of intermediary and acting as the unifying and coordinating medium. Without such contacts clinical research runs the risk of becoming either very narrow and specialized or superficial and diffuse, through lack of the necessary technical advice and supervision. The fact of the matter is that we should recognize no sharp distinction between "clinical" and other forms of research in medical science.

It is further necessary to point out that it is the function of medicine, not merely to raise questions to be elucidated by the chemist, the pathological physiologist, or the pathological anatomist, but to impress upon all these its peculiar nosographical method. Much work loses a good deal of its scientific value and practical importance because the observations are isolated from their clinical context. It is not sufficient to study abnormalities of structure, function or chemical composition without reference to the condition of the patient as a whole and to the natural history of the disease, otherwise it becomes impossible to differentiate real diseases and syndromes, or to gain information of value in prognosis. Mackenzie did a great service in reminding the modern medical world of the importance of such considerations, and in this he followed worthily in the tradition of Hippocrates and Sydenham. One has only to be reminded of the confusion which has arisen in connexion with the functional and anatomical classification of renal affections, or the difficulty in appreciating the significance to be attached to a large number of irregularities revealed by the electrocardiograph, or the interpretation to be placed upon the biochemical findings in glycosuria or renal disease to realize the necessity for correlating all these observations with the clinical course of the disease. On the other hand, one may cite the advances made by the application of the nosographical method in differentiating hyperpiesia and malignant hypertension from the scrap heap of renal disease, of the syndrome of coronary thrombosis from *angina pectoris*, or of renal glycosuria from diabetes. The triumphs of this method in the past, in the hands of men like Bright, Stokes, Adams, Graves, Laënnec, Duchenne, Brettoneau and a host

of others, at a time when pathological anatomy had not yet definitely parted company with clinical medicine, should make us pause before casting it lightly aside in these days of experimental medicine. The experimental production and cure of disease, the isolation of ætiological factors and the application of special methods for the qualitative and quantitative determination of function have all been of inestimable value in advancing medical science, but, so far from enabling us to dispense with nosographical method, they should for the most part be regarded as means to the same end, namely, the complete description of diseases, their nature, characters and course. The need of the present is to apply the nosographical method in connexion with experiment as well as observation and autopsy.

Similar considerations apply with regard to the use of the various laboratory diagnostic methods. We often hear these spoken of as "scientific" as opposed to physical signs and symptoms, which are referred to as "clinical." This confusion and false antithesis could only arise through a total misunderstanding of the nature and purpose of these tests. A positive Wassermann reaction or hyperglycæmia is as much a clinical sign of disease as a presystolic murmur or spots on the abdomen. Moreover, they are to be evaluated in the same way, namely, by considering them in relation to all the facts of the case and to the chronological sequence of events. The question of whether a test is carried out in proximity to the bedside or not may be a matter of practical convenience, but it does not affect the scientific character of the investigation.

Besides laboratory facilities, certain other arrangements as affecting the hospital are necessary for the prosecution of medical research. It is desirable that the entire clinical material of the hospital should be made available for research, while there should, in addition, be a special observation ward or unit provided with a diet kitchen and a close proximity to the laboratories, to which patients could be drafted, if necessary, from other parts of the hospital for special kinds of investigation.

In choosing a research staff for a department of medicine, due consideration must be given to the function of the members of that staff and their future careers. First, there should be a number of part-time qualified assistants, junior men who have a reputation to make and whose ultimate aim is consulting practice. They should, in addition to doing their general work in the hospital, interest themselves in some special branch of medicine and if possible be attached to a special out-patient clinic. Such special clinics—cardiological, neurological *et cetera*—with their follow-up systems, would constitute a valuable source of material for both research and teaching, as well as a good training ground for the future consultant. They would deal with ambulatory patients only, and all patients would be referred to them either from the general out-patients' department or from the general wards of the hospital and back again. In this way most of the advantages of specialization may be retained without the corresponding evils. These part-time men should cease to hold office when the demands of consulting practice become too great to enable them to devote sufficient time to investigation.

At the opposite pole from the part-time clinicians would be certain whole-time research workers representing the fundamental or ancillary sciences, for example, chemists, physicists or physiologists who have elected to devote their services to medicine. These men would not be necessary in the larger scheme outlined, but in a self-contained department of medicine they are essential in order to assist and guide investigation on the technical side. They should not, or need not, be clinicians at all. Experience shows that, with but few exceptions, medical men who have turned biochemists seldom attain the high degree of proficiency necessary to enable them to do really fundamental work. Again, physiologists in the guise of clinicians tend to neglect their patients and are as a rule bad therapeutists and diagnosticians. Hence men in this category should not be given full charge of beds in the hospital, but should obtain clinical facilities for working out their own problems through the professor of medicine who would be responsible for the clinical care and selection of patients. The status and outlook of these special men is a matter of importance. They would have little prospect of being promoted to chairs of chemistry or physiology, or, if they did, they would be lost to medicine, while, unlike physicians, they could not look forward to private practice or chairs of medicine. The solution of the difficulty would seem to lie in the direction of creating special research professorships for this class of investigator, while the professor of medicine would stand in relation to them as *primus inter pares*.

Between the pure clinicians aiming at consulting practice and the pure laboratory workers, there would be an intermediate class of medical men whom one might designate as academic physicians. The academic physician, as typified by the whole-time professor of medicine, should be a man fitted by training in the fundamental sciences and by experience as a practical physician to appreciate the aims, method and spirit of both practice and research, to inspire and promote investigation, both at the bedside and in the laboratory, to undertake the care of patients and to teach the principles or institutes of medicine. Men aiming at careers of this kind should be made whole-time or almost whole-time assistants to the professor of medicine, and they should be expected to devote their main activities to research and teaching. In any case, they must be prepared to cast their bread upon the waters until such time as they may be fortunate enough to be promoted to chairs. After a probationary period they should be paid salaries intermediate between those in the two former categories to tide them over the lean years.

To summarize, my conception of the new whole-time chairs of medicine is that they are essentially chairs of the institutes of medicine, that is, of the

principles of medical science in all its aspects, clinical and laboratory, observational and experimental. Catholic in outlook and synthetic in function, it should be their mission and purpose to coordinate the various activities which come within the purview of what we have defined as medical science. What we require to do at the present time is to revive, and at the same time to redefine, the institutes of medicine.

It is now necessary to inquire how the method and spirit of science may pervade the teaching of medicine. Is it possible to teach medicine by observation and experiment? It may be accepted as a general principle that experience which is actively sought and actively acquired is much the most valuable and lasting in its effect. It follows as a corollary that spoon feeding should be eschewed and that as much as possible should be learned by practical experience. Hence the system of apprenticeships, whereby a man becomes an integral part of the organization of the hospital or of the laboratory, provides a training of the most excellent kind. It is impossible, however, for anyone to acquire a science through his own resources and experience, hence a certain amount of theoretical instruction is necessary. The question is, what is the best method of imparting it and how far is it possible in teaching to adhere to the principles of scientific method? From a purely didactic point of view, it is simpler to begin by discussing causes and then deducing or describing effects, but in clinical medicine we are always observing certain effects whose causes we seek. The natural method and that most conformable with scientific procedure, is to begin with the observation of the patient and later to explain the problems presented by him with the aid of pathological anatomy, pathological physiology and the other sciences. In doing so, due historical perspective should be preserved, the facts and hypotheses should be made to appear as they may have done in the mind of an investigator, and the explanation should often become evident even before it is formally stated. The aim should be to foster the spirit of inquiry, to awaken interest, stimulate the imagination and inspire a love of the art. We should not teach theories as dogmas, nor should we have everything simplified and carefully prepared in "tabloid" form, as do the exponents of popular science.

In order to carry out this method the lecture theatre must be conveniently situated in close proximity both to the wards and to the pathological museum, and, in addition to the showing of patients and pathological specimens, every device in the way of projection apparatus *et cetera* should be employed to facilitate the concrete presentation of the subject. A considerable amount of clinical material would have to be made available for demonstration purposes and this could be attained by placing at the lecturer's disposal the entire clinical material of the hospital, including out-patients and special clinics. Even if this were done, it might be objected that in a systematic course of medicine it would be impossible to show examples of everything. This is quite true, but fortunately it is not necessary. Nor need the systematic character of the course be abandoned on that account, as is done in Germany, Austria and Holland. In these countries such systematic theoretical instruction as may be given is delegated to junior assistants, while the main teaching medium is the clinical lecture. These clinical lectures are models of their kind, but the subjects chosen are entirely dependent upon what cases of interest happen to be in the wards. While they may be intelligible to the senior student or to the post-graduate, they are often quite over the head of the junior student, who may begin his course of medicine by listening to a lecture on disseminated sclerosis followed by one on typhoid fever or on gastric ulcer.

The difficulty can be overcome by a suitable arrangement of the course of instruction. For this purpose the course should be divided into three phases, corresponding to three different ways of cross-indexing one's knowledge, namely: (*a*) pathological physiology, general semeiology and general therapeutics; (*b*) nosography, special semeiology and specific therapy; (*c*) differential diagnosis and prognosis.

In the first, disease would be considered from the point of view of how altered structure and function manifest themselves in symptoms and signs, and how disturbances of function can be remedied. This would form the natural transition from anatomy and psysiology to clinical medicine and pathology. General aspects of ætiology would also be dealt with at this stage. In the second phase specific diseases would be dealt with from the point of view of their ætiology, natural history and specific therapy, together with the special signs and symptoms associated with them. This ground having been covered, the student would be ready to proceed to the final phase which should be devoted to the study of differential diagnosis and prognosis.

In the first phase, owing to the general nature of the subject matter, great demands would not be made upon the resources of the hospital for illustrative material. By the time the second is reached, the exact order in which specific diseases would be dealt with would cease to be a matter of importance and could be regulated by the available cases in the wards, and the same may be said of the final phase which is concerned with differential diagnosis. Nor, for that matter, is it necessary (even if it were desirable) to illustrate or to describe all diseases, and the student should be made to realize that reference to text books and original literature is part of the technique of acquiring a science.

These three phases should form the underlying basis of the curriculum in the three final years, which we may call the clinical years, and the various subjects should be arranged and correlated from this point of view. Medicine and surgery should be run as a joint course, extending over three years and, together with obstetrics, gynæcology and pædiatrics, should form the backbone of the curriculum in these years. In the first clinical

year medicine and surgery should be correlated with the ancillary sciences of pathological anatomy, bacteriology and pharmacology on the one hand and with clinical instruction on the other. The latter should bear a definite relation to the subject matter of the lectures and the main emphasis should be upon clinical methods, physical signs and elementary clerking. Therapeutics should be brought under medicine, as the restitution of function can most conveniently be dealt with along with disturbances of function. By correlating these various subjects, overlapping can be avoided and a great deal of time saved, so that the total number of lectures can be reduced and more time made available for clinical work. For example, while the bacteriologist dealt with infection, the pathological anatomist would take up inflammation and repair, the surgeon wounds and sepsis, the physician fever and pain, the pharmacologist antiseptics and analgesics and so on. From time to time combined demonstrations on all these aspects of the subject should be given. In fact, a teaching museum, arranged in sections to correspond with these comprehensive demonstrations, would be of great value in a medical school.

In the second clinical year, when specific diseases would be taken up, the various special subjects dealing with specific diseases—fevers, tuberculosis, venereal diseases—would come in. None of the minor specialties, including dermatology, ophthalmology, diseases of the ear, nose and throat, and orthopædics, should be dealt with in isolated courses or treated as specialties for the purposes of undergraduate instruction. The various lecturers in these subjects should be called upon at the appropriate time to give a strictly limited number of lectures in conformity with, and as an integral part of the general course of medicine and surgery. The main instruction in the specialties should be of a practical character, given in the special clinics, to which the students should be attached in groups during this year, while continuing to attend the general medical and surgical wards. In conformity with the general scheme a feature of the training in clinical medicine at this period should be the memorization of data with regard to patients and the giving of case histories and the results of examination from memory. The student should also make the cases he studies the starting point of his reading. In this way the foundations of clinical experience are laid.

The third clinical year should be entirely devoted to clinical work. An ideal to be aimed at in the future would be to have a series of undergraduate internships in clinical medicine, surgery and obstetrics. The only lectures to be attended at this time should be those on differential diagnosis, given by the clinical lecturers in medicine and surgery at the hospital. Casualty conferences should also be held from time to time so that the possibilities of error in diagnosis may be appreciated. As regards work in the wards, the student would be working largely alone and would be made more definitely a part of the hospital organization. His case records, instead of adhering slavishly to the schedules provided for the guidance of junior students, should be rather more discursive, containing less irrelevant detail, and include a commentary. Similarly, in more formal bedside clinics, the senior student, having by this time obtained a knowledge of diseases as well as of semeiology, should learn to follow up the clues as they arise, so as to arrive with reasonable rapidity at a conclusion. As the main emphasis should be upon differential diagnosis, the undergraduate, at the end of his second clinical year, should be required to prove his proficiency in the physical examination of patients before being allowed to join the senior clinic.

Physicians who undertake the responsibility of teaching should regard their obligations towards the student under their charge in the light of the best Hippocratic traditions. Bedside clinics provide an almost ideal field for training in scientific method and for cultivating the scientific spirit. Here also the art may be learned by practical experience and through the example of those who are already masters. The wise teacher will not indulge in lengthy explanations nor should he be too ready to demonstrate the points of the case himself. Rather should he adopt the "obstetric" method of drawing out the mind of the student by asking questions and getting him to discover as much as possible himself. By being thrown upon his own resources the student learns to observe and think independently, to be on the alert to recognize differences, to exercise his imagination in an endeavour to perceive the bearings of his observations and, above all, to use his critical faculties. A strict discipline must be observed: inductions must be correct and logical, carefully distinguishing between observation and inference, while accuracy of description and precision in the use of terms are to be insisted upon. Discussion of the diagnosis usually affords an excellent exercise in both inductive and deductive reasoning.

A most important but much neglected part of clinical training is case taking. It would be difficult to conceive of a better means of cultivating methodical habits, of getting into the way of making observations accurately and of recording them faithfully and systematically, a discipline which is essential in all scientific pursuits.

An education conceived along such lines may be expected to inculcate something of the spirit and method of science; but would it give any insight into research? Perhaps to a limited extent and in a sort of counterfeit way. But let a man undertake a real piece of investigation; let him frame a single original hypothesis and subject its consequences to the arbitrament of observation or experiment and then, and then only, will he become one of the initiated.

Practice is essentially the sphere of the man of action rather than that of the man of thought. Here the emphasis is upon art rather than upon science. Nevertheless, the practitioner, in examining a patient and arriving at a diagnosis, employs the

scientific method in which observation, induction, deduction and observation or experiment follow in rapid and repeated succession. The method, although scientific in form, differs, however, from that of original research. The practitioner is, for the most part, concerned with the application of the known. He makes inductions, but his hypotheses are like ready-made garments rather than original creations of the *salon*, and it is a question of finding out which fits best. The assumption is that the end of the inquiry will be the identification of one of a number of things presumably known and only indirectly may this lead to the discovery of the unknown; it is a question of a choice of given alternatives. While, therefore, the method followed by the diagnostician may claim to be scientific, it is somewhat less intellectually stimulating than that of the original investigator.

Diagnosis, as Friedrich Müller remarks, is the peculiar art of the physician. It consists for the most part in the recognition of particular significant patterns of symptoms and signs. With experience such skill may be acquired that it becomes almost second nature, so that people speak of "clinical instinct." It does not consist merely in an understanding of the physiological mechanisms underlying symptoms; it is the grouping, sequence and relative weight to be attached to symptoms which are of even greater importance. Those who are more physiologists than clinicians are apt to lose themselves in the details of the physiological processes involved in the production of particular symptoms and signs, instead of viewing the case in all its aspects. Once more it is a harking back to the tendencies which distinguished the rival schools of Cnidos and Cos; to the differences between those who attend to symptoms and the refinements of topical diagnosis and those who envisage the patient, his surroundings and the course of his disease as a whole; between the pathological anatomists and physiologists on the one hand and the nosographists on the other. We have already had occasion to refer to these two points of view in connexion with medical education. In one's medical pilgrimage it is well to begin at Cnidos, but to end at Cos.

The true physician should possess not merely a knowledge of disease and skill in diagnosis and treatment, but he should be a student of human nature. He must be possessed of insight, understanding, tact, sympathy and all those qualities which spring from the finer feelings of man.

> For who can always act? but he
> To whom a thousand memories call,
> Not being less but more than all
> The gentleness he seemed to be, . . .
>
> (Tennyson)

The doctor is not merely a scientific expert; the relationship between doctor and patient is an ethical one. The practitioner should be a man interested, not only in the science of disease, but in his fellow creatures, in humanity.

> Ην γὰρ παρῇ φιλανθρωπίη πάρεστι καὶ φιλοτεχνίη.
>
> (Hippocrates)

We have traced the method and spirit of science and of art. What sort of ideal are we to strive for? In this harmony of head, hand and heart we may find the expression of our common aspirations: love of truth, love of the art and love of humanity.

SOME RECENT ADVANCES IN THE TREATMENT OF PULMONARY DISEASE.[1]

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Tuberculosis.

FOR nearly fifty years the causal agents of pulmonary tuberculosis and pneumonia have been known. During that time there has been a steady and gradual improvement in the mortality rate from tuberculosis. In the year 1900, out of every hundred thousand people in New South Wales, eighty died from pulmonary tuberculosis; in 1929 this rate had been reduced to forty-seven per hundred thousand. What factors were responsible for this diminished death rate? Firstly, a more widespread knowledge as regards prophylaxis of the disease, secondly, better methods in its earlier recognition; thirdly, improved therapeutic measures. If it were possible to collect all tuberculous patients in sanatoria, where they might have the benefits of modern methods of treatment, there is little doubt that the mortality rate would shrink still further.

Great strides have been made in the early diagnosis of pulmonary tuberculosis. The correct interpretation of mild constitutional symptoms before the appearance of physical signs, the aid received from both the radiologist and the pathologist have all been determining factors.

Artificial Pneumothorax.

By what means has the treatment of tuberculosis advanced? Residence in a sanatorium is still generally accepted as the essential condition which should underlie all other treatment, but, unfortunately, it is available to only a small percentage of patients. If sanatorium residence be unavailable, then rest with the best hygienic measures possible should be employed at home for a period of eight to twelve weeks, at the end of which time there should be a general improvement of symptoms as regards weight, cough, sputum, pain *et cetera*. If this improvement be not forthcoming and the disease be unilateral in distribution, artificial pneumothorax should be induced. This form of treatment has now been accepted as one of the most valuable in the treatment of tuberculosis and is now carried out early in the disease. It is very extensively practised in America, England and the Continent. It should also be employed in an attempt to check an otherwise uncontrollable hæmoptysis or when there is cavitation with unilateral disease and

[1] Read at a meeting of the New South Wales Branch of the British Medical Association on October 30, 1930.

tubercle bacilli are found in the sputum or in the treatment of bilateral disease, when involvement of the second lung is confined to the apex above the second rib. Occasionally complete collapse of the lung is impossible on account of pleural adhesions; some form of phrenic nerve surgery, such as squeezing, cutting or avulsion, is then helpful.

The induction of artificial pneumothorax is now a relatively safe and easy procedure, thanks to the modern apparatus, of which I prefer Heaf's. It causes little discomfort to the patient and can be carried out in sanatoria or at home. Reports from the Loomis Sanatorium in America[1] and the Wald Sanatorium in Switzerland,[2] in both of which it has been in extensive use during the past fourteen years, show that 42% of patients treated by means of artificial pneumothorax are alive and their condition satisfactory four to fourteen years after the inception of treatment, whilst in 25% of the total number the disease was arrested. Comparison between patients subjected to artificial pneumothorax and those untreated by this procedure revealed that there were three times as many of the former class alive as the latter.

Such brilliant results cannot be expected in the treatment of bilateral disease, but it is quite remarkable the manner in which some patients with infection in both lungs respond to treatment by means of artificial pneumothorax. Reactions from artificial pneumothorax may be classified into four types, namely: (i) General nervous reaction; (ii) congestive reaction of the opposite side; (iii) pleural reaction, that is, effusion; (iv) "refill" reaction, the principal symptoms of which consist of rise of temperature shortly after the refill to as high as 39·45° C. (103° F.), a sensation of uneasiness or tension on the insufflated side and malaise; these symptoms disappear within twenty-four hours.

The pleural effusion may fill only the costo-phrenic angle; the amount is not then abnormal. On the other hand, it may occupy half to three-fourths of the entire pleural space. These large effusions contain tubercle bacilli but seldom become purulent. Such effusions obviate the necessity for frequent refills and apparently are salutary, for when they arise the prognosis seems more favourable. They appear to exert an inhibitory action on the bacilli. Eventually the effusion is absorbed, the pleura becomes markedly thickened, yet the lung expands and, best of all, the tuberculous process is arrested.

Postural Rest.

During the probationary period of eight to twelve weeks between making the diagnosis and the decision as to whether artificial pneumothorax is to be employed, the chance of improvement should not be left solely to measures of rest and hygiene. I have employed "intensive postural rest" as advocated by Gerkler and Weigel.[3] The patient lies on the affected side, on a pillow shaped like a mattress with side wings. Its length is 50 centimetres (20 inches), width 25 centimetres (10 inches) and thickness 7·5 centimetres (three inches). Commencing with one hour a day the time is increased by half an hour daily up to a maximum of sixteen to twenty hours. At first there may be slight increase of local and constitutional symptoms, but these soon subside. As the thoracic muscles become fatigued by working against a pressure equal to half the body weight, there is on the affected side a cessation of all movement except that of the diaphragm. This "postural rest" greatly diminishes the possibility of aspiration of infective material into the lung and does not promote the formation of pleural adhesions. It is contraindicated when debility is pronounced or severe hæmoptysis is is present and if the disease be on the left side it should not be employed for one hour after a meal. So much improvement may take place during this probationary period that no further treatment need be instituted and the lesion heals.

Other Remedial Measures.

Of the other remedial measures, heliotherapy, which plays such a large part in the treatment of surgical tuberculosis, is not only disappointing, but frequently harmful. Tuberculin is of value principally in the treatment of chronic tuberculosis when the lung is slowly undergoing fibrosis and needs stimulation to complete this process. The use of gold salts led to considerable disappointment in the earlier years of this decade and in some instances has even been harmful, either on account of the accumulation of the drug in the system or severe toxic reactions. Quite recently, on account of increased knowledge better results have been reported. "Sanocrysin" and "Solganal B" are now the salts most in favour. By their actions the sputum is rendered free of tubercle bacilli, the fever reduced and the cough diminished. Gold salts should not be employed when the pulmonary disease is complicated by abdominal tuberculosis.

For several years past the blood calcium content of patients suffering from tuberculosis has attracted attention. Its variation is directly proportional to the serum globulin content, and in inverse ratio to the quantity of inorganic phosphates. Normally the blood calcium content is 10·1 milligrammes per 100 cubic centimetres of serum. As the result of investigation I have found this figure is reduced to an average of 9·8 milligrammes per 100 cubic centimetres in the early stages of tuberculosis and still further reduced as the disease becomes advanced. An estimation of 9·3 milligrammes per 100 cubic centimetres of serum has always indicated in my experience that the end was not very far off. I have tried by various means to raise this lowered blood calcium content. Calcium salts by mouth and intravenously, alone, and in combination with parathyreoid gland extract administered by the oral or intramuscular route have all failed. When, however, calcium salts were administered in combination with cod liver oil, the rise in the blood calcium content was surprising. It not only became normal, but remained so, and the improvement in the symptoms and the disease as a whole was *pari passu*.

It appears to me that in some way the vitamin in the cod liver oil activates the calcium metabolism to absorb and retain the calcium administered. The preparation used was "Oleocal," which is a combination of "Tricalcine" and cod liver oil. The dose given was three or four tablets three times a day. It produced a remarkable increase in weight, as much as 8·5 kilograms (nineteen pounds) in six months in a patient whose weight had remained stationary for eighteen months. The systematic treatment of tuberculosis knows no limit. One of the most distressing symptoms is night sweats. Scot Skirving many years ago taught that tissue resorption was in inverse ratio to digestive absorption and that tuberculous patients who took food or milk at bedtime were relieved of a good deal of this excessive sweating. In addition, sweating has been controlled in my experience by a daily intramuscular injection of five cubic centimetres of a 25% solution of magnesium sulphate for a week or ten days.

Diet.

A change has taken place even in regard to diet. No longer is forced feeding with its two litres (four pints) of milk and numerous eggs daily the vogue. A mixed varied diet of 2,000 or 2,500 calories in the early stages of the disease, with a normal ratio of protein, carbohydrate and fat radicles is now more generally regarded as suitable.

As a result of these improvements in the treatment of tuberculosis the average sufferer now has an expectancy of life of five or six years, whereas a quarter of a century ago the disease ran its course in an average of three to three and a half years.

Pneumonia.

Acute lobar pneumonia is still the most fatal of all acute diseases. The mortality rate has not greatly altered during the past thirty years. Statistics in this respect are somewhat misleading. Public hospital returns show that the mortality rate from lobar pneumonia varies from 15% to 30%; this figure is somewhat higher than the actual rate throughout the general population, as the patients admitted to hospital are affected by the disease in its most virulent form; many of them are moribund at the time of admission. Again a large number is sent to hospital relatively late in the course of the disease, when the actual moving of them adds to the gravity of the prognosis, whilst the natural defensive powers of the average hospital patient are lower than those of people more happily situated in life.

It is now generally believed that in the treatment of lobar pneumonia the administration by mouth of any medicine with the exception of digitalis is of little value. Fraser,[4] Cohn[5] and many others have shown that digitalis does support the heart, slowing its rate and increasing its efficiency. Most other drugs are not only valueless, except psychologically, but frequently harmful inasmuch as they impair digestion.

Pneumococcus Antiserum.

Let us consider for a moment what therapeutic methods are employed in attempting to reduce this high mortality rate. First, Cole,[6] Park,[7] Cecil[8] and their coworkers at the Hospital of the Rockefeller Institute have given an extensive trial to antipneumococcus serum and their results show a lowering of the death rate from thirty to twenty *per centum*. The bacteriologists have gone to a lot of trouble and by pertinent and close investigation of the habits of the pneumococcus have succeeded in subdividing it into four types. Unfortunately this has not been of much help to the clinician unless he has a bacteriologist in close cooperation with him. The technique of differentiating the type of pneumococcus is somewhat involved and takes from six to twenty-four hours. For this reason antipneumococcus serum is of little benefit except in large and well equipped hospitals. To obtain satisfactory results the serum must be specific to its own type of pneumococcus and not polyvalent. It must be given intravenously in large doses and at frequent intervals varying from eight to twelve hours. As much as 100 to 150 cubic centimetres should be given on the first day and from 300 to 400 cubic centimetres during the course of the disease. Considerable experience and judgement are required in its use and on the whole it has not generally yielded satisfactory results. Moreover, there is considerable reaction after its use; such reaction may be: (i) Anaphylactic in character, due to the foreign protein contained; (ii) thermal, characterized by a rise in temperature followed by temporary defervescence; (iii) serum sickness or disease, which occurs from ten to fourteen days after its administration, and is exceedingly embarrassing to a patient about to convalesce.

The best results have been obtained when the pneumococcus belongs to Type (1); there has been no improvement when the organism has belonged to Type (3) or (4). Felton[9] and Park[10] have lately concentrated the serum, but it is questionable whether there has not been a considerable loss of the active substance in the process.

Oxygen Administration.

Cyanosis which was thought to be due to circulatory disturbance, is now believed to be due to anoxæmia.[11] Efforts have been made to overcome this by the early administration of oxygen by means of the nasal catheter, tent or oxygen chamber. The optimum saturation point in the blood aimed at by one observer was 80% and when this was obtained 75% of the patients recovered, whilst when this saturation point could not be maintained 57% of the patients died. On these facts it may be argued that oxygen, when absorbed, has a beneficial influence on the outcome of the disease.

Specific Drugs.

From time to time drugs that would destroy the pneumococcus have been tried to influence the prognosis. Of all published results optochin,[12] a

quinine derivative, has given the best results, but although it is bactericidal to the pneumococcus *in vitro* and to some degree in the blood, its use has been largely discontinued owing to its highly deleterious effect on the eyesight. For some years past I have used iodine in varying combinations with results that have satisfied me as to the improvement in the mortality rate. During the last seven years in which one hundred consecutive patients suffering from lobar pneumonia have been treated by the administration of iodine, only two deaths have occurred. It has been given intravenously as peptonate of iodine, equivalent to 0·08 gramme (1·25 grains) of iodine, once or twice a day, according to the severity of the symptoms. The temperature drops to 37·25° C. (99° F.) or under within an average of five days, when the administration of the drug is stopped. There has not been any protein shock or iodine poisoning or other untoward complication. The preparation used has been "Iodone," manufactured by Robin, of Paris.

Coincidentally with the use of this drug a leucocyte count of the blood has been made daily. At the onset of the disease the average number of leucocytes per cubic millimetre has been 25,000, with a preponderance of polymorphonuclear cells. After the injection of "Iodone," mononuclears have increased from one or two *per centum* to as high as eight or ten *per centum*. In this way it would appear that not only does the drug have a bactericidal action, but also stimulates phagocytosis. In both cases of fatal disease the total leucocyte count never rose above twelve thousand per cubic millimetre and there was no improvement in the mononuclear cells. Although the series is small, the results are sufficiently encouraging to justify publication. No attempt was made to classify the type of pneumococcus and with the exception of digitalis and oxygen no other drugs were used. The use of this drug in a great majority of cases on the first or second day of the disease, the better environment in the semi-rural atmosphere, combined with the greater natural defensive forces of the patient, may have influenced the lowering of the mortality rate.

Bronchiectasis.

Bronchiectasis, if not one of the most fatal diseases, is certainly one of the most crippling and detrimental to the patient. Dilated bronchi with weakened walls, destruction of endothelium and fibrosing changes in the surrounding lung tissue are pathological changes difficult to alter. It has now been amply demonstrated that bronchiectasis is often closely associated with chronic paranasal sinus disease and its onset frequently dates from an unresolved influenza. The classical picture of fœtid sputum, clubbed fingers, emaciation *et cetera* is surely the final stage of this devastating disease and if it be allowed to reach this stage, little improvement can be expected from any therapeutic agents.

The injection of "Lipiodol" provides now an easy and certain method of diagnosis, so that it may be confidently anticipated that this disease will be seen and recognized at an earlier stage, and, let us hope, efficient treatment instituted. A complete survey of the whole of the respiratory tract, upper and lower, must be made; treatment aimed at the cure of any sinus disease should be instituted at the beginning. In my own hands artificial pneumothorax on the side on which the disease is more advanced in early cases has given encouraging results, simultaneously producing improvement of the opposite side. Cough and sputum have largely disappeared and after cessation of the treatment there has been no recurrence of the symptoms. My small series of patients and the comparatively short time they have been under observation and treatment prevent me from making any further deductions on the prognosis. In some instances the patient has been too ill to submit to radical operation for disease of the upper part of the respiratory tract, while conservative drainage and repeated lavage have produced sufficient improvement to render any further treatment of that region unnecessary. In the treatment of advanced bronchiectasis the bronchoscopist has come to our aid by instituting drainage and lavage. Emptying of the cavities by posture seems also to give some relief. Phrenic nerve surgery has been of assistance when adhesions have prevented the induction of complete artificial pneumothorax.

In conclusion, great as is the toll on life taken by respiratory diseases, the outlook for the future is distinctly promising. Even if there does not occur any dramatic discovery such as the specific antitoxin of diphtheria, still it is not too much to hope that pulmonary disease will occupy a far lower position on the list of the causes of death than now, as a result of the use of the methods described and others yet to be discovered.

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SOME NOTES ON THE VALUE OF X RAYS IN PREGNANCY.[1]

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MY intention with these few notes is to indicate to the general body of medical practitioners how the Röntgenologist may be of assistance to them in the ordinary routine of their day's work when dealing with cases of pregnancy that are not straightforward. No attempt will be made to deal with the subject fully from a radiographic point of view. (I speak, not as a Röntgenologist, to Röntgenologists, but as a Röntgenologist, in general terms, to the medical practitioner.) Various radiographic procedures will be touched on, but technique will not be dealt with. Again, I do not propose to sustain your interest with a display of films of pregnancy in its various aspects, normal and abnormal.

It seems to me, after many years of practice as a radiographer, that it is not generally known or understood in what ways X rays may be of assistance, for the amount of work that we are called upon to perform in this sphere is so little. However, at times many vexed questions can be settled and a definite diagnosis given radiographically before a clinical diagnosis is possible.

I shall limit my remarks to the following points, dealing briefly with each:

1. The pelvis: Its measurements.
2. The fœtus: (*a*) The establishment of pregnancy, the age at which the presence of the fœtus may be detected by X rays and the confirmation of pseudocyesis; (*b*) the presentation, the possibility of multiple pregnancies *et cetera*; (*c*) deformities and lesions of the fœtus detectable *in utero*, for example, anencephaly, bone lesions as *fragilitas osseum*; (*d*) the death of the fœtus.
3. Contrast media tests: (*a*) Rubin's test; (*b*) "Lipiodol" injection of the uterus and tubes.

The Pelvis.

Various methods have been devised to estimate the measurements of the bony pelvis, some very complicated. More recently, with the advent of the Potter-Bucky diaphragm, Roberts, in England, and Thomas and Heublein, in America, devised the method of the vertical transmission of rays through the centre of the plane of the pelvic inlet. By this method there is obtained an evenly enlarged shadow of the outline of the pelvic inlet; by measuring the distance of the target of the tube from the film and from the plane of the pelvic inlet (the upper border of the symphysis approximately) the measurements of the pelvic inlet can be calculated. In this way various deformities are very well demonstrated. The value can be shown by the following example.

Mrs. H. had had a very difficult and delayed labour in Ceylon with her first child and was therefore referred to have her pelvic measurements calculated, as the question of the advisability of future pregnancies was raised. The pelvic measurements were shown radiographically to be normal and the trouble was obviously not with the bony passage. A subsequent successful pregnancy with easy labour confirmed the X ray findings.

The Fœtus.

Diagnosis of Pregnancy.

For many years the general opinion was that the fœtus did not cast a shadow until about the fourth month of intrauterine life. I admit that I held this view for a good many years. Later, with the advent of the Potter-Bucky diaphragm, Martin Jungmann showed that the Röntgen visualization of the fœtus is possible from the eighth or ninth week onwards. It has not been my lot to examine a great many women during early pregnancy. At the tenth or eleventh week the fœtal bones are very apparent. If now we can demonstrate the presence of a fœtus at the eighth or ninth week, then we might possibly be able to lessen the number of deaths from ruptured ectopic gestation; if we could demonstrate the existence of a fœtus in an abnormal position when the diagnosis lay between ectopic gestation and some other pelvic mass, then the earlier positive diagnosis of pregnancy with subsequent operation before rupture might improve the mortality rate. I do not know if such a thing would be possible. I put it forward as a suggestion, for, as mentioned earlier, these women are not sent to the radiographer, the usual view being that before four months no fœtal shadow can be obtained by X rays.

Presentation, Multiple Pregnancies et Cetera.

Most of the pregnant women referred for X ray examination are sent in order that the presentation or the existence of multiple pregnancies may be determined. They are usually big, fat women, possibly with hydramnios; the clinical diagnosis is difficult or uncertain on account of the size, distension *et cetera*. These patients require no further discussion. We are dealing with a fœtus towards full term and the fœtal shadow or shadows leave no doubt. The question of pseudocyesis is easily determined and demonstrable to the patient and her associates.

Abnormalities of the Fœtus.

Defects and abnormalities of the fœtus may be quite easily established. More and more cases of diagnosis of anencephaly by means of X rays, while the fœtus is *in utero*, are being reported. The diagnosis is obvious, the well-formed body and the tiny head leaving no doubt as to the condition.

[1] Read at a meeting of the New South Wales Branch of the British Medical Association on October 30, 1930.

Death of the Fœtus.

Another type of patient occasionally referred to the radiographer is the one in whom the death of the fœtus is suspected. I understand that this may be a difficult question to decide clinically in some instances. Radiographically, shortly after the death of the fœtus, an overlapping of the cranial bones occurs, due to the decrease in the pressure of the cerebro-spinal fluid and such overlapping may be well seen on the films and at once establishes the diagnosis.

Contrast Media Tests.

Finally we have the contrast media tests, first, by air, the Rubins test, to establish the question of patency or not of a tube; and, secondly, the injection of "Lipiodol" or other contrast substance into the uterine cavity. This demonstrates the contour of the uterine cavity and defects in it, the contour and patency of the tubes and the site and type of obstruction, if any. Work has been done with this medium actually to demonstrate pregnancy in a very early stage, but this seems to entail an unnecessary risk.

The "Lipiodol" injection is to the uterine cavity and tubes what the opaque meal and enema are to the alimentary tract, and the pyelogram, cystogram, cholecystogram *et cetera* are to their corresponding organs, and it has opened up a further field for diagnosis.

As I said before, I am putting forward these brief notes to show how in many doubtful cases an X ray examination may be of some value. I am not attempting to write a thesis on the radiographic aspect of pregnancy.

Reports of Cases.

CONGENITAL MALFORMATIONS OF THE HEART AND GREAT VESSELS: REPORT OF TWO CASES.[1]

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Coarctation of the Aorta.

Clinical Notes.

THE patient was admitted to the Royal Prince Alfred Hospital complaining of vomiting and breathlessness. The vomiting had commenced seven days previously, the vomitus being at first foul smelling and later greenish. Breathlessness of a severe grade had been present for seven weeks. It was experienced mostly in the evening, when the patient had to rest beside the road while going home from work. Some degree of breathlessness had, however, been present for twenty years, and he was frequently "winded" after a hard day's work. Four years ago this became so severe that he had to go into hospital, but after a time he was able to go back to work.

On examination the patient was a well built and moderately well nourished man with œdema of both ankles. The vessels in the neck were seen to pulsate vigorously. The temperature was 36·1° C. (97° F.), the pulse rate 96 and respiratory rate 40.

[1] Read at the first annual reunion of the Royal Prince Alfred Hospital Residents' and Ex-Residents' Association, October 7 to 11, 1930.

The apex beat was situated in the fifth left intercostal space 15 centimetres (six inches) from the mid-line. A systolic thrill was felt. No right cardiac dulness was noted. The heart sounds were loud and the second aortic sound was accentuated. Systolic murmurs heard at the mitral area and the aortic area were both conducted over wide areas of the chest. No diastolic murmur was heard, but the pulse was of a Corrigan type and was regular except for occasional extra systoles. The systolic blood pressure was 180 and the diastolic pressure 90 millimetres of mercury. Râles could be heard at both bases, but, apart from this, no abnormality of the respiratory system was noted. The liver was found to be greatly enlarged and was tender to palpation. No abnormality was detected on examination of the nervous system. The specific gravity of the urine was 1·020, the urine was acid, acetone was present and a faint cloud of albumin was detected, but no blood, pus or sugar. The Wassermann reaction was not obtained. The blood urea content was 28 milligrammes *per centum*.

Though the vomiting yielded to treatment and the patient soon felt much better, the œdema became more pronounced. On the fifteenth day in hospital he became increasingly cyanosed and died on the following day.

Post Mortem Findings.

At autopsy there was œdema of the legs and scrotum. About two pints of clear straw coloured fluid were present in each pleural cavity and there were about two pints in the peritoneal cavity.

The ascending aorta and the arch of the aorta as far as the *ligamentum arteriosum*, are thick walled, comparatively rigid and dilated. Just distal to the point of attachment of the *ligamentum arteriosum* there is a well marked constriction which corresponds in position to a diaphragm-like septum which here obstructs the lumen of the aorta (see Figure I). This septum is perforated

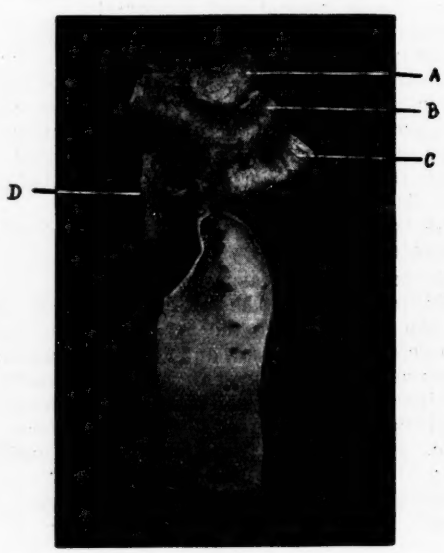

FIGURE I.

Coarctation of the aorta. A = innominate artery. B = left common carotid artery. C = left subclavian artery. D = portion of the left pulmonary artery which is joined to the aorta by the *ligamentum arteriosum*. The constriction in the aorta is immediately below the attachment of the *ligamentum arteriosum* to the aorta.

by a small oval aperture which is just under two millimetres in diameter. The *ligamentum arteriosum* is not patent. The aorta, distal to this part, is relatively narrow and thin walled. The higher intercostal arteries are larger than usual, as are the majority of the lumbar arteries.

The transverse arteries of the scapula, which in this case arise from the third part of the subclavian artery, are thick walled and much dilated. The internal mammary arteries are also much dilated, being about the size of a normal brachial artery.

The heart is much enlarged, weighing 750 grammes (twenty-five ounces). The right atrium is much dilated and the wall is hypertrophied. The tricuspid valve ring is dilated, admitting five fingers. The cusps are thin. The right ventricle is much dilated and the wall hypertrophied. The pulmonary valve shows no abnormality. The left atrium is dilated and its musculature is slightly hypertrophied.

The mitral valve is somewhat dilated and admits three fingers. The cusps are slightly thickened. The left ventricle is very much hypertrophied, especially at the base, but at the apex the wall is only eight millimetres thick. The cavity is much dilated and the *trabeculæ carneæ* are flattened.

There is chronic endocarditis of the aortic valve. The anterior and the left posterior cusps are fused together and the bridle between them is elongated so that the cusps may easily be retroverted. The free borders of these cusps are thickened and cord-like and are continuous with each other so that, at first sight, the cusps appear as one. The right posterior cusp is enlarged and thickened.

The coronary arteries show sclerosis and have rigid and thick walls.

The lungs are somewhat œdematous and congested and show well marked "brown induration." There is marked passive venous congestion of the liver and spleen and the kidneys are much engorged.

Discussion.

Coarctation of the aorta is fully described and discussed by Maude Abbott in the section on congenital cardiac disease in Osler's "Modern Medicine."[1]

Two main types are described. In one, known as "Bonnet's infantile type," the constriction of the aorta is diffuse and is situated proximal to the junction with the *ductus arteriosus*. The ductus in these cases is patent and the aorta, which receives its blood from this structure, appears to be a continuation of the pulmonary artery. This type is usually seen in the new born.

The other type, of which the case here reported is an example, is known as "Bonnet's adult type." Here the constriction in the aorta is sharp and is usually situated distal to the *ligamentum arteriosum*, which is not patent. The aorta receives blood from an extensive collateral circulation.

Skoda put forward the theory that the constriction was caused by the presence in the aortic wall of tissue similar in nature to that found in the *ductus arteriosus*. He suggested that this aberrant tissue contracted simultaneously with the obliteration of the ductus, thus producing an abrupt constriction near the junction with this structure.

The condition is not very rare. Maude Abbott collected 237 cases from the literature. Of these cases 82 were of the infantile type and 155 were of the adult type.

Acknowledgement.

I am indebted to Dr. C. G. McDonald for permission to publish this case.

Origin of the Aorta and Pulmonary Artery from the Right Ventricle Associated with a Vestigial Left Ventricle, Dextrocardia and Transposition of Viscera.

Clinical History.

Four days before admission to hospital the patient, P.McC., a female, aged six months, had become ill with a cough, fever and sleeplessness and refused nourishment. Three days later her stools became green in colour, but there was no vomiting. Prior to this she had had no illnesses and no respiratory distress. She had been a normal, full-time baby and was breast fed.

On examination the patient was a well nourished and developed child who was very cyanosed and obviously very ill. There was no clubbing of the fingers or toes. The temperature was 38·3° C. (101° F.), the pulse rate was 160 and the respiratory rate 60.

Respirations were described as "grunting, short, sharp and rapid." Inspiratory retraction was marked in the basal portions of the chest. Vocal fremitus and resonance were increased over both lungs and the percussion note was impaired. The breath sounds over both lungs were between a harsh vesicular and a harsh bronchial type and were accompanied by bronchial râles.

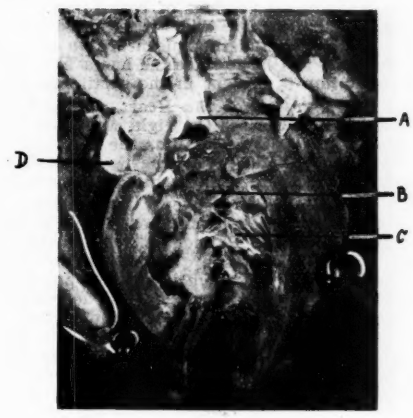

FIGURE II.

CASE II. A photograph of the heart from the ventral aspect. The right ventricle, the pulmonary artery and the aorta have been opened. A = aorta. B = the muscular band separating the aortic and pulmonary orifices. The band has been cut across and two *chordæ tendineæ* may be seen attached to it. C = tricuspid valve. D = pulmonary artery.

The apex beat was felt on the right side of the chest in the fourth intercostal space, 6·25 centimetres (two and a half inches) from the mid-line. The heart sounds were, to a great extent, obscured by the râles.

The abdomen was full. An area of dulness without breath sounds at the base of the right lung corresponded clinically to the liver dulness, so that transposition of the liver was not recognized. On the left side a band of

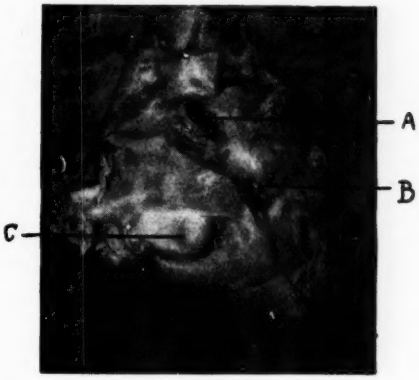

FIGURE III.

CASE II. A photograph of the heart from the dorsal aspect. The left atrium has been opened. A = *foramen ovale*. B = vestigial mitral valve. C = posterior part of right atrium.

dulness five centimetres (two inches) wide intervened between the pulmonary and the gastric resonance. A sweat rash was present on the skin of the abdomen.

The patient's condition showed no improvement on the following day and she died two days after admission.

Post Mortem Findings.

At autopsy no definite cyanosis was noted. The apex of the heart is situated in the right side of the chest. A large cardiac notch is present in the anterior border of the right lung. Both lungs contain two main lobes and in both there is a tendency for the upper lobe to be divided into two unequal parts by a fissure which, however, is longer on the left side. There is irregular consolidation in the basal portions of both lungs.

The larger lobe of the liver is situated on the left side and the stomach and the spleen are on the right side of the abdomen. The large intestine has a long mesentery. An intussusception is present in the small intestine.

Detailed Examination of the Heart. Viewed from the front, a ventricle comprises the right and inferior portion of the heart and a large atrium, the left and superior portion. Behind the large atrium is a smaller atrium which corresponds to the left atrium of a normal heart.

The left atrium can be seen only from behind. It is a muscular structure which terminates on the right side in an auricular appendage. Pulmonary veins enter it on both sides and it communicates by a *foramen ovale* with the other (the right) atrium which is situated anteriorly to it. Caudally there is a small hemispherical depression in the wall of the left atrium. This is considered to be a vestigial mitral valve since, in the floor of the depression, there are two minute and delicate valve cusps. A section through this shows a rudimentary left ventricle about 1·5 centimetres deep and just wide enough to admit a wooden match. Minute *chordæ tendineæ* join the valve segments to rudimentary papillary muscles.

The right atrium is large and much more voluminous than the left. The auricular appendage is directed towards the right side. On opening the atrium the *foramen ovale* is seen as a slit-like opening bounded above by a semicircular muscular band which, during contracture of the atrium, should have closed the foramen. The inferior boundary of the foramen is a straight muscular band.

The superior *vena cava* opens into the left supero-lateral angle of the atrium and the inferior *vena cava* enters the atrium in the left infero-lateral angle.

The tricuspid valve is situated on the left side of the right ventricle to the left side of the orifices of the pulmonary artery and aorta.

The right ventricle is a large thick-walled muscular chamber from which both the aorta and pulmonary artery arise.

The aortic and pulmonary valves are separated from each other by a thick muscular band which projects into the ventricle for a distance of eight millimetres. This is continuous with a large muscular trabeculum which projects from the posterior wall of the ventricle. To it are attached two *chordæ tendineæ* which connect it with the anterior cusp of the tricuspid valve.

The ascending part of the aorta is situated to the left side of and slightly anterior to the pulmonary artery. The arch courses directly backwards and is joined by a patent *ductus arteriosus* to the pulmonary artery at its bifurcation. The descending aorta is situated on the right side of the œsophagus.

The pulmonary artery is comparatively large and lies on the right side of the ascending part of the aorta and slightly posterior to it.

Discussion.

The main abnormality in the heart described is the vestigial condition of the left ventricle and mitral valve and the origin of the aorta from the right ventricle. It will be noted that the interventricular septum has not failed to develop nor has it failed to unite with the interauricular septum.

The interventricular septum has not, however, in its development made union with the septa that divide the *truncus arteriosus* and the *bulbus cordis*.

To understand the abnormality it is necessary to consider the division into two parts of the *bulbus cordis*. This in the primitive heart is the part situated between the common ventricle and the *truncus arteriosus*.

As the common ventricle and atria are divided into right and left halves a similar division takes place in the *truncus arteriosus* and the bulbus.

In the case of the bulbus this division is formed by the union of two endocardial projections called the right and left bulbus cushions, so that the bulbus is divided into right and left portions. The right portion, as Keith[2] points out, ultimately becomes a large part of the infundibulum of the right ventricle, while the left part is incorporated into the vestibule of the aorta. Keith describes the manner in which the cushions become invaded by muscle from the bulbo-ventricular junction, and shows that the two cushions ultimately form the right and left septal bands in the right ventricle of the fully developed heart. The remainder of the septum which separates the ventricles is formed by a muscular fold which grows up from the inferior part of the common ventricle.

During early stages of development the dividing *bulbus cordis* is situated on the right side of the dividing common ventricle. In order that the septa dividing these two structures may unite with each other the bulbus should move towards the left side to override the ventricular septum.

In the heart under consideration it appears that this movement to the left has not taken place so that the part of the interventricular septum which is developed from the septum of the primitive bulbus has not united with the part that grows upwards from the inferior part of the ventricle. The muscular band which separates the aortic and pulmonary orifices is taken to be that part of the interventricular septum which develops from the right and left bulbus cushions and which in a normal heart would form the right and left septal bands.

Acknowledgements.

I am indebted to Dr. J. J. C. Cosh for permission to publish this case and to Dr. A. H. Tebbutt for helpful criticism and advice. I have also to thank Mr. Newson for the photographs.

References.

(1) Maude E. Abbott: "Congenital Cardiac Disease," Osler's "Modern Medicine," Third Edition, 1927, Volume IV, page 612.

(2) Arthur Keith: "The Fate of the Bulbus Cordis in the Human Heart," *The Lancet*, 1924, Volume II, page 1267.

Reviews.

THE FEMALE GENERATIVE ORGANS.

A. VON FEKETE gives as the reason for the publication of his book the need for a summary of the literature which has arisen since the war concerning the relationship of the female genital organs with the body in general and especially with the endocrine system.[1] He has made a very careful study of the literature coupled with a critical survey of the results so far reported. All stages in the life of woman—infancy, puberty, menstruation, pregnancy and the menopause—have been exhaustively treated and their relationship with other systems fully correlated. Particular attention has been devoted to the modern views on ovarian secretion and the control exercised by the anterior lobe of the pituitary gland. A short summary of treatment of each condition is included. As is only too common with German writers, no reference is made to British investigators, notably Wilfred Shaw and Crew, and all the credit is reserved for their compatriots.

[1] "Die Funktion der Weiblichen Geschlechtsorgano und ihre Beziehungen zum Gesamtorganismus für Ärzte und Studierende," by Alexander V. Fekete; 1930. Berlin: S. Karger. Crown 4to., pp. 285. Price: Mk. 10.70 net.

The Medical Journal of Australia

SATURDAY, JANUARY 31, 1931.

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CANCER OF THE STOMACH.

PEOPLE are told by members of the medical profession that if they seek medical advice in the early stages of malignant disease, the chances of obtaining a cure are considerable. This is, of course, wise teaching; and it must be realized that if every malignant focus were removed or irradiated as soon as it was macroscopically recognizable or as soon as it produced symptoms, deaths from malignant disease would number but a small percentage of their present total. Unfortunately it sometimes happens that when a patient presents himself to his medical attendant on account of some departure from his normal state of health, apparently of a minor nature, the correct diagnosis is not made; and both patient and medical attendant suddenly become cognizant of the real state of affairs when the time for effective energetic action is passing or has passed. Since this has been known to happen with such conditions as epithelioma of the skin, it is obvious that it must occur much more frequently with so difficult and obscure a condition as cancer of the stomach. The question naturally arises as to what extent this state of affairs exists and what may be done to remedy it.

A partial answer to the first of these two questions is supplied by a report from the Cancer Division of the Department of Health of Detroit by Dr. H. C. Saltzstein and Dr. D. J. Sandweiss, published in *Archives of Surgery*, July, 1930. The report is concerned with 365 consecutive deaths from cancer of the stomach. Of the patients concerned 213 were observed in hospital and 152 were not. The series covered one and a half years and included all deaths registered as having been caused by cancer of the stomach in a population of 1,730,000. The figures are probably typical of most communities. In the Commonwealth of Australia during 1928 the number of deaths from cancer was 6,010 and from cancer of the stomach and liver 2,165. Of the latter number 1,333 occurred in males and 832 in females. During this year the estimated population was 6,336,786 persons. The proportion of deaths in Australia is slightly higher than that found in Detroit—0·03%, as compared with 0·02%. Of course, the Australian figures include those of cancer of the liver. The most appalling fact brought to light by the Detroit inquiry was that only 3% of patients had the chance of cure that follows successful performance and recovery from a radical resection operation. The Detroit patients were divisible by their symptoms into two groups (data were obtainable in only 287 instances). In 24·7% the conditions followed a long period of indigestion; in 75·3% the condition started suddenly from previous good health. In the latter group the symptoms were mainly referable to the upper part of the alimentary tract and included indigestion, pain in the epigastrium, a history of ulcer and vomiting. It must be concluded that gastric carcinoma is not diagnosed in its early stages and part of the responsibility for failure in diagnosis must be laid at the door of the medical practitioner.

In regard to the remedy, it would appear to be a simple matter to tell everyone that indigestion in middle life is the precursor of cancer and that persons so affected should submit themselves to examination. If every middle-aged person with dyspepsia were examined by a medical practitioner, cases of malignant disease of the stomach would still be missed, because methods of diagnosis are

far from perfect. This is not to be thought of at present, for more harm than good would result from the starting of a cancer scare in the minds of middle-aged dyspeptics. Until the laity can be told with conviction that diagnostic methods are so reliable that they will reveal cancer whenever it is present, such teaching must be given judiciously and more attention must be paid to diagnosis and diagnostic methods. To begin with, the medical practitioner must remember that indigestion, particularly if of sudden onset and not promptly relieved by ordinary therapeutic measures, may be due to malignant disease. The recognition that malignant disease may possibly be present is the first important step. X ray examination must be undertaken and its limitations borne in mind. Chemical tests must be carried out. Gastroscopy is yet in its infancy and may be expected to be of use later on. Finally, physicians must not shrink from recommending exploratory operation at the hands of a surgeon whose knowledge of clinical pathology is more than superficial. Laparotomy is a small price to pay for certain discovery of a carcinoma and its early removal. Even by all these methods it will not be possible to discover every malignant growth of the stomach, but if they are carefully applied, more patients will be given a chance of cure and mortality will be reduced.

Current Comment.

THE TOXICITY OF VITAMIN *D*.

IT is well known that untoward symptoms may arise in the human subject after the administration of large doses of cod liver oil or even after moderate doses, if they are continued for an undue length of time. These manifestations are largely cardiac, consisting of discomfort, tachycardia and other disturbances of the heart's action. Lesions of the cardiac muscle, particularly of the auriculo-ventricular bundle, have been described and it has even been suggested that instances of heart failure in later life may be traced back to undue medication with cod liver oil in childhood for rickets or other disorder. Laboratory animals given excessive doses of the oil, although they gain in weight, may have evidence of microscopical lesions in the digestive tract, parenchymatous organs and skeletal system, with fatty and pigmentary degeneration of and lime deposits in the cardiac muscle. On suspending the oil, some of the lesions heal, but others do not. Cod liver oil contains vitamins *A* and *D*—the latter more particularly. The toxic symptoms have been attributed to hypervitaminosis *D*.

In 1927 Rosenheim and Webster found that ergosterol subjected to ultra-violet rays gained powerful antirhachitic properties. At one time it was considered that cholesterol was the substance which thus became antirhachitic. It is now, however, proved that the associated ergosterol, and not chemically pure cholesterol, becomes antirhachitic when treated with ultra-violet light. Ergosterol was first described as being obtained from ergot. It occurs also in the integument of animals and in green plants and is sometimes called phytosterol. Irradiated ergosterol is a most effective agent in preventing and curing rickets. It is sold commercially under a variety of names. J. B. Duguid[1] has summarized our knowledge of the toxic effects of vitamin *D* overdosage, that is, hypervitaminosis *D*. Ergosterol is the precursor of vitamin *D*. It can be readily prepared from yeast and thus vitamin *D* may be prepared in concentrations immeasurably stronger than those found in Nature. Both in children and laboratory animals pathological conditions follow overdosage with irradiated ergosterol, that is, vitamin *D*. However, there is a wide margin of safety between the therapeutic and toxic doses. Daily doses of 0·0001 milligramme will cure rickets in a rat in ten days; daily doses of one milligramme will kill a rat in three weeks.

The pathological effects of large doses in laboratory animals are profound wasting, roughened coats, anorexia, diarrhœa, rapid and laboured breathing, with death in from six days to three months. Massive deposits of calcium are found in the heart, arteries, kidneys, walls of the alimentary canal and elsewhere. The earliest calcification usually develops in the kidneys with casts in the tubules. The renal epithelium is destroyed and calcified. The bladder may contain calculi. The aorta is the first of the arteries to be affected; irregular calcification of the media occurs. The calcification spreads to the smaller arteries, including the renal. The veins are not involved. The spleen is pallid and shrunken. There is diminution of the pulp cell with condensation of the reticulum and connective tissue and intense hæmosiderosis. The lymphoid tissue of the Malpighian bodies (as of the body generally) is diminished. The thymus is also atrophied. It may be noted that the atrophy of the spleen and thymus are in marked contrast to the hypertrophy seen in rickets. In addition there is anæmia with a diminution of the lymphocytes. Premature calcification of the growing cartilage arrests development. In the pregnant female there are retardation of growth and skeletal deformities in the fœtus. Hypercalcæmia and hyperphosphatæmia occur. At first there are increased serum calcium and blood inorganic phosphorus coincidently with excessive excretion of calcium and phosphorus, especially by

[1] *The Lancet*, November 1, 1930.

the kidneys. This, with the associated anorexia, results in diminished calcium and phosphorus retention in the body. Contrary to the general opinion, the percentage absorption of calcium, except possibly in the early stages, tends to fall in hypervitaminosis *D*. Perhaps hypercalcæmia and pathological calcification result from mobilization of calcium from the bones. What aids calcification if in normal amount may cause decalcification if it be in excessive quantity. Some observers, however, have not found resorption in the bones or osteoporosis coincident with tissue calcification. It will be noted how closely parallel the manifestations of hypervitaminosis *D* are with those of hyperparathyreoidism. On withdrawing the drug, some observers have seen no evidence of healing or resorption of calcium from the arterial lesions, but others have found both. In any case restoration of normal metabolic balance is slow.

There would appear to be an optimum dosage of vitamin *D*. Underdosage and overdosage respectively cause diminished or increased figures of the blood calcium and/or phosphorus. Great variation of dosage has been used in experiments. The amount of vitamin *D* produced by irradiating ergosterol varies. Three substances are successively formed and destroyed. The first is vitamin *D*; the others are inactive. The shorter ultra-violet rays destroy vitamin *D* if their application be unduly prolonged. Irradiation destroys as well as produces vitamin *D*. The amount of ergosterol used is no index of the vitamin present. The amount of the vitamin has been variously estimated as from 35% to 55%. The toxicity of irradiated ergosterol disappears concurrently with the destruction of vitamin *D* by over-irradiation. It would seem evident that the toxic symptoms are produced by vitamin *D* itself and are not due to the possible formation of any poisonous by-products. Non-irradiated ergosterol and ergosterol resinized without the formation of vitamin *D* are both non-toxic. Ergosterol irradiated in the absence of any solvent will produce specific ill effects. If activated in oil or in alcohol, in either case yielding a high vitamin *D* content, irradiated ergosterol may produce hypervitaminosis of equal severity. A high calcium diet favours early production of lesions, which are delayed by a calcium-free diet. A high calcium and low phosphorus diet predisposes to toxic effects from vitamin *D*. Vitamin balance is also of importance. If vitamin *B* be deficient, focal necrosis of the heart muscle may be induced by large doses of cod liver oil. Thus deficiency of other vitamins may increase the toxicity of vitamin *D*. A high marmite (vitamin *B* complex) diet gave increased tolerance to cod liver oil containing vitamins *A* and *D*. Duguid states that young animals tolerate "Vigantol" (irradiated ergosterol) better than older ones, but a contrary assertion has been made by others. Rhachitic animals are more tolerant of high doses than are healthy ones. Toxic effects are greater in castrated animals and those whose spleen has been removed.

Toxic effects are also seen in human beings from overdosage with irradiated ergosterol. In tuberculous children have been noted loss of appetite, emaciation, vomiting, albuminuria with hyaline casts, hæmaturia and high blood calcium and phosphorus content. At autopsy have been found parenchymatous nephritis and calcification as in animals.

It is absolutely essential that some method of standardization of the various preparations of irradiated ergosterol be enforced. Reliance has largely been placed on the dosage indicated by the manufacturers. The most logical method is to estimate the potency in terms of protective or curative rat units. Coward adopted as a standard a preparation of irradiated ergosterol of which 0·0001 milligramme in daily doses heals standard rhachitic rats in ten days. The adoption of the rat unit would place experimental work on a common basis. The Medical Research Council Committee on Accessory Food Factors has announced its intention of adopting the rat unit and maintaining a similar standard preparation for use in assaying preparations of vitamin *D* in England. Their antirhachitic unit will be defined as the antirhachitic potency of a quantity of their preparation corresponding to 0·0001 milligramme of the ergosterol used in its preparation.

It will be seen that the indiscriminate use, without supervision, of either cod liver oil or irradiated ergosterol may be fraught with hazard and must give way to scientifically controlled administration. It is true that there is a wide margin between the therapeutic dose and toxic dosage, but the exercise of caution remains essential.

PROLAPSE OF THE BREAST.

THE normal contour and resiliency of the breast have been lost in many young women of today. Lilian K. P. Farrar is undoubtedly right when she lays the blame at the feet of Dame Fashion.[1] She points out that when girls adopted a dress for sport, copied from the wardrobes of their younger brothers, the "boyish form *brassière*," with its tight, non-elastic constriction of the chest was the logical corollary. The fashion in sports clothes soon extended to evening dress until the flat chest became *de rigueur*. The breast is attached only loosely by areolar tissue to the fascia overlying the muscle. The result of unyielding pressure is atrophy and prolapse. In the most dependent portion the breast tissue forms nodules and these have been mistaken for growths and removed. Prevention is better than cure, but even when prolapse has taken place recovery may result from wearing a proper elastic supporting *brassière*. Since women would "rather be dead than out of the fashion," the only thing to do is to change the fashion.

[1] *The Journal of the American Medical Association*, November 1, 1930

Abstracts from Current Medical Literature.

MORBID ANATOMY.

The Effect of Vaccine Lymph on the Central Nervous System of the Monkey.

P. A. CLEARKIN (*The British Journal of Experimental Pathology*, October, 1930) discusses the changes occurring in the monkey, *Cercopithecus albigularis* after the intradermal administration of vaccine lymph. In the majority of instances injection of vaccine lymph has failed to cause in the central nervous system of laboratory animals lesions comparable to those seen in the post-vaccinial encephalitis of man. In the author's experiments the monkeys were caught wild and used within a few weeks of capture. They were given plenty of room and food such as they had been accustomed to in their wild state. The virus used was obtained from a local (*Dar-es-Salaam*) strain of small pox and was maintained by passage through rabbits and calves. The monkeys were killed at periods varying from nine to twenty-one days after the injection. In all the monkeys which had received moderate and large doses of lymph there was some degree of adherence of the *dura mater* to the cranial vault; adhesions were dense and firm after large doses. Congestion of meninges, hemispheres, pons, medulla and cervical part of the spinal cord was pronounced. Occasionally even the lumbar portion of the cord was affected. No such lesions were observed in the unvaccinated control monkeys. Microscopically there were observed proliferation of the endothelial lining of capillaries of the meninges of the brain and cord and perivascular collections of cells in the cortical substance. Demyelination of white fibres in the region of vessels was observed in some instances after moderate and large doses of vaccine. The author suggests that the monkey *Cercopithecus albigularis* is more susceptible to the effects of the virus than the laboratory animals used in other experiments. This monkey lives in the woods and only visits the haunts of man in search of food; it is therefore very unlikely to have acquired human diseases or to have developed a resistance to them. The author remarks that the lesions observed in his experiments are histologically similar to those observed in the central nervous system of persons affected with post-vaccinial encephalitis. He expresses the opinion that this supports the view that vaccinia is the cause of post-vaccinial encephalitis.

Rheumatic Pericarditis with Polypoid Formations.

MILTON G. BOHROD (*Archives of Pathology*, July, 1930) reports the occurrence of a peculiar polypoid mass in the pericardium of a man, aged twenty-seven years, who had died of rheumatic heart disease. There was old valvular disease of the aortic and mitral valves with calcification of the vegetations. There was in addition a more recent endocarditis. The left auricle and both ventricles were greatly hypertrophied and all cavities were dilated, especially the right auricle. The pericardium was chocolate brown in colour. The sac was lined with shaggy fibrinous material which could be stripped off with ease. There were adhesions between the two layers of the pericardium; these were very firm on the anterior aspect of the heart along the line of the interventricular septum. The sac contained a quantity of cloudy, brownish fluid. At the point where the parietal pericardium was reflected from the right auricle posteriorly, there hung a hydatidiform mass, 7·0 centimetres in depth by 8·5 centimetres in width by 2·5 centimetres in thickness. This mass consisted largely of numerous polypoid bodies, some of which were solid, but most of which were filled with altered blood. Over the surface of the visceral pericardium of the auricle were tiny brownish projections, 0·5 to 3·0 millimetres in height; most of them were solid, but some contained old blood. There were several such projections on the pericardium of the anterior surface of the heart also. The cystic polypoid bodies were lined with a single layer of flattened cells. The solid bodies contained clot in various stages of organization or old hyaline connective tissue. Macrophages containing blood pigment were to be found in the walls of the polypoid bodies. The author considers the mass to be a peculiar result of the healing of a rheumatic hæmorrhagic pericarditis.

Psittacosis.

G. HASWELL WILSON (*The Journal of Pathology and Bacteriology*, October, 1930) describes the *post mortem* appearances of the organs of a man who died of psittacosis. The lesions of psittacosis in birds are to be found mainly in the gastro-intestinal tract, whereas in man the lungs are the organs most profoundly affected. The individual whose body was examined by Wilson was a man aged fifty-eight years. The upper lobe of the left lung was consolidated and firm; the lung was not increased in size nor marked by the ribs. The consolidated part of the lung was dusky red, dry and almost crumbling; the granular appearance of the cut surface seen in ordinary lobar pneumonia was absent, though the pneumonia was of the lobar type. The unconsolidated part of the lung was poorly aerated and deeply congested; there was some œdema at the base and the posterior border. The lining of the bronchi was intensely congested; the larger bronchi were empty, but the smaller contained a small amount of turbid blood-stained fluid. There were similar appearances in the right lung, though the consolidation here was not so extensive. There were a few petechial hæmorrhages beneath the visceral pleura and a small area of recent pleural exudate at the base of the right lung. The liver was pale and had the appearances of cloudy swelling and fatty infiltration. There was evidence of cloudy swelling in the kidneys also. The spleen was enlarged and very soft. The stomach and intestines were œdematous. The mucous membrane of the stomach and large intestine was congested, but there was no congestion of the lining of the small intestine. There was no lesion of importance in the brain or its membranes. Microscopic examination of the lungs revealed occasional small hæmorrhages and a few capillaries blocked by hyaline thrombi. Most of the exudate appeared to be fibrinous in character; the consolidated area was largely composed of a network of fibrinous material. The cells in the exudate were for the most part mononuclear in type and appeared to have been derived from the lining of the air vesicles. Polymorphonuclear cells were scanty and were present in the hæmorrhagic areas rather than in the exudate. At the base of the right lung there was a small area of bronchopneumonia which presented appearances suggestive of a secondary infection. There were extreme engorgement of the spleen and numerous hæmorrhages; polymorphonuclear leucocytes were scanty. There was parenchymatous degeneration of the liver and spleen; there was extensive desquamation of the epithelium of the renal tubules. The general picture was that of septicæmia. The author suggests that the changes in the lungs may have been due to intense toxic damage rather than an inflammatory reaction. The leucopenia was a striking feature, but the occurrence of a terminal bronchopneumonia was evidence that the body was still capable of a leucocytic response to a pyogenic infection. The author points out the difference between the lung changes associated with psittacosis and influenza as it appeared in the 1918 epidemic. In conclusion he remarks that in sections of the lung of persons who have died of influenza, numerous bacteria may be seen, whereas bacteria cannot be cultivated from the psittacosis lung unless a secondary infection is present.

Tuberculosis of the Stomach.

E. R. CULLINAN (*The Journal of Pathology and Bacteriology*, October, 1930) describes the *post mortem* appearances of the body of a man, aged fifty-six years, who had died of tuberculosis of the stomach. He mentions that whereas the intestines are often attacked by tuberculosis, tuberculous disease of the stomach is a rarity. The body was emaciated. The lungs were studded with miliary tubercles and there was evidence of recent pleurisy on either side. There were no old foci in the lungs or bronchial glands. No lesion was found in the intestines save in the caecum, where there was an ulcer, the long diameter of which lay transversely to the length of the gut. On the lesser curvature of the stomach, near the

cardiac end, was a shaggy, soggy mass which did not have the appearances of a malignant or simple ulcer of the stomach. Along the lesser curvature was a chain of enlarged lymphatic glands. Histologically the ulcer in the caecum did not appear to be tuberculous. Microscopical examination of a section of one of the glands along the lesser curvature revealed giant cell formation and other appearances of tuberculous disease. The normal tissues of the wall of the stomach at the site of the lesion were largely replaced by inflammatory fibrous tissue containing numerous spherical necrotic foci. The mucous membrane in the area had been completely destroyed and portions only of the muscular coat remained. There were no giant cell systems, but in the necrotic areas were vast numbers of tubercle bacilli. It was quite obvious that the lung lesions were more recent than the lesion in the stomach. The author suggests that the stomach lesion was the primary focus.

MORPHOLOGY.

Brain of the Rhesus Monkey.

FREDERICK D. GEIST (*Journal of Comparative Neurology*, August 15, 1930) gives an account of a complete series of sections of the brain of the monkey (*Macacus rhesus*) prepared by a very simple modification of the iron-hæmatoxylin technique, in which a striking contrast between medullated fibres and other elements of the brain is obtained. The essential part of the work consists in the presentation of drawings of selected sections with the anticipation that they will serve as a readily available guide for those engaged in experimental investigation of the brain of the monkey. In the selection of sections to be used as illustrations the criterion has been that of utilizing such sections as bear sufficient differences in appearance of certain structures or contain new structures not shown in the preceding section.

The Reflex Activities of a Decerebrate Animal.

L. J. POLLOCK AND LOYAL DAVIS (*Journal of Comparative Neurology*, August 15, 1930) describe results obtained in cats decerebrated by the anæmia method. This method of decerebration consists of two steps: First, the ligation of the basilar artery at any desired level and, secondly, the ligation of the carotid arteries. The level of section obtained by the production of anæmia may subsequently be accurately determined by the peripheral intravenous injection of vital stains, such as methylene blue. Without exception decerebrate rigidity, similar to that described by Sherrington, has resulted after the ligation of the arteries. The authors give a description of a cat decerebrated by the method of producing anæmia; they discuss "righting reflexes" and "standing reflexes," cases of the coexistence of "righting reflexes" and decerebrate rigidity, the reciprocal action of tonic innervation and other reflexes, the influence of the cerebellum upon the posture of a decerebrate animal *et cetera*. Illustrations and bibliography are given.

Behaviour Reactions and Development of the Spinal Cord.

WILLIAM F. WINDLE (*Journal of Comparative Neurology*, August 15, 1930) gives an account of correlated physiological and anatomical observations on development with the purpose of throwing new light on the function of certain centres and tracts of the nervous system. He studied a series of kittens from birth to maturity and followed the normal behaviour reactions and responses to stimuli. The spinal cords of the same animals were studied histologically. Suggested correlations with the physiological data are presented. The first week of life is characterized by more or less generalized responses and poorly controlled reactions; correlated with this there is found in the spinal grey matter a meshwork of fine fibres arranged for the most part in transverse and oblique planes. No pericellular plexuses and no end bulbs are yet present. The third week is characterized by the acquisition of control over small muscle groups in the forelegs, while from the structural standpoint a pronounced proliferation of the fine fibre meshwork of the grey matter appears. Pericellular plexuses and occasionally end bulbs are now found in the cervical region, but in the lumbar segments little evidence of advance is observed.

The Pars Nervosa of the Bovine Hypophysis.

PAUL C. BUCY (*Journal of Comparative Neurology*, August 15, 1930) describes the structure of the *pars nervosa* of the hypophysis of cattle. A new method of impregnating the specific cells is given. These cells are called "pituicytes." The nerve fibres which arise in the supraoptic nuclei and descend into the stalk and infundibular process, and the dense connective tissue network are described in addition to the "pituicytes" of the *pars nervosa*. These latter cells are not numerous and are interspersed between the relatively large masses of nerve fibres and the connective tissue.

The Utriculo-Endolymphatic Valve.

E. F. HOFFMAN AND T. H. BAST (*Anatomical Record*, September 25, 1930) give an account of the literature concerning the existence of the utriculo-endolymphatic valve which is described as a highly cellular, valve-like connective tissue flap covered by a columnar epithelium. It guards the opening of the utriculo-endolymphatic duct into the utricle. The authors find that the presence of the valve is a definite and constant feature of the membranous labyrinth of the mammalian ear and that this fact tends to strengthen the probability of its functional use. The authors, however, are unable to give an indication of the importance or function of the valve.

Occurrence of Vaginal Plug in a Chimpanzee.

O. L. TINKLEPAUGH (*Anatomical Record*, September 25, 1930) reports the occurrence of a vaginal plug in a chimpanzee similar to that commonly known to occur in rodents. It has never before been reported as occurring in primates. The author gives an account of the nature and function of vaginal plugs.

A Study of Human Ova from Large Follicles.

E. ALLEN, J. P. PRATT *et alii* (*American Journal of Anatomy*, June 15, 1930) record a careful census of the number, size and condition of follicles in human ovaries at varying times in the menstrual cycle. More than sixty ovaries obtained at operation were studied. One oocyte obtained was in the process of meiotic division. This was recovered from a follicle ten millimetres in diameter on the fourteenth day following the onset of the previous menses. The data obtained in this study support the conclusion that radical selective elimination of ova occurs in human ovaries during late stages of growth of the follicles. An extensive bibliography and thirty figures are given.

A Surface Structure in Normal Nucleated Erythrocytes.

SAVAS NITTIS (*Anatomical Record*, September 25, 1930) has found in studying the erythrocytes of animals in which these cells are found in circulation in the nucleated form, that a "stigma" or dot may be observed in each cell when supravital staining is employed and the specimens are kept in the wet condition. This structure apparently does not fit into any category of the many "granules" previously described. It has been suggested by previous authors that it is of the nature of a centrosome. The present author finds another "stigma" at the opposite end of the cell and speculates on the existence of a minute circulatory system within the erythrocyte.

Endochondral Ossification.

G. S. DODDS (*Anatomical Record*, September 25, 1930) gives an account of the method by which cells of cartilage during the process of endochondral ossification become arranged in rows parallel to the long axis of the bone. The formation of rows is seen at its best development in the shafts of rapidly growing bones, such as those of the legs and feet, during the time when they are growing in length by the steady advance of ossification toward the cartilaginous ends of the bones, both before and after the appearance of the epiphyseal centres of ossification.

Special Articles on Diagnosis.

(Contributed by Request.)

XXX.

INFECTIONS OF THE FALLOPIAN TUBES.

In an acrimonious discussion in the pages of *The British Medical Journal*, between Spencer Wells and Lawson Tait, on the diagnosis and treatment of adnexal diseases, Spencer Wells wrote: "Who can discover anything about these remote structures in the living body?" To this Lawson Tait replied: "I have taught scores of medical men to accurately diagnose abnormal Fallopian tubes and ovaries."

No one now denies Lawson Tait's contention that such diagnosis is not only possible, but comparatively easy. The mistakes that are still made, and, in the writer's experience, these are not very uncommon, are due to neglect of that complete investigation which every patient has the right to expect from the doctor to whom she entrusts her life. To palpate the abdomen and, on detecting rigidity and tenderness, perhaps most marked in the right iliac fossa, to jump to the hasty conclusion that the appendix is at fault, is to exhibit a lack of reasonable skill and care and therefore may conceivably render the surgeon liable to legal proceedings. Such superficial examination will certainly, sooner or later, cause him much worry and regret.

I have heard of instances of ruptured pus tube, ovarian cyst and torsion of pedicle and, strange as it may seem, ectopic gestation, being mistaken for appendicitis. In two of the patients fatal results followed. Such untoward occurrences would be almost impossible if the recognized methods of diagnosis were carried into effect.

In my opinion it pays to take trouble with the history: Has there ever been a similar attack?

The appendicitis patient will often say there have been other slight attacks of pain. The ectopic gestation patient generally says no such agonizing pain has ever been experienced. The salpingitis patient may or may not have had similar attacks.

Where did the pain start? About the umbilicus in appendicitis, lower down in salpingitis.

Has the pain any relation to the menstrual period? Has a period been missed, even one day overdue? Salpingitis is often determined by the physiological congestion and lowered resistance of the menstrual period. Pain following a missed period should be assumed to be ectopic gestation until the contrary is proved.

Has there been irregular coitus? This information for what it may be worth. Marital coitus only will not necessarily exclude salpingitis.

Has there been a yellow discharge which began acutely? Has there been a recent abortion or visit to an abortionist? This, of course, while being presumptive evidence of a streptococcal infection, would not necessarily exclude an ectopic gestation. I have known of several attempts to induce abortion when the pregnancy was tubal, not uterine.

Has there been any recent general infectious disease, such as measles, scarlet fever, mumps, enteric fever, *et cetera*? We know salpingitis not uncommonly has its origin in such general infections.

Is there anything to suggest a tuberculous focus?

The physical examination should be not less thorough.

The aspect of the patient will often throw light on the problem; the blanched face of the victim of a ruptured ectopic gestation or the anxious expression in grave infection.

In acute infection of the tubes one will expect to find constitutional reaction in the shape of raised temperature, pulse and respiration and leucocytosis, while the peritoneal irritation will express itself in vomiting. Inspection of the abdomen will show distension in proportion to the extent of peritoneum involved. There will be more thoracic respiration than normal. Palpation will disclose tenderness and rigidity most marked over each iliac fossa. The patient lies with the thighs flexed on the trunk in order to minimize the pressure of the parietes on the inflamed structures beneath.

No diagnosis for any intraabdominal lesion should be arrived at without a vaginal or rectal examination. Inspection of the vulva will often give valuable information, such as purulent discharge, redness, pus in Skene's or Bartholin's glands.

In acute pelvic conditions it is not necessary or desirable to make a bimanual examination. All the information required can be obtained by gliding the finger over the vaginal vault, noting the presence or absence of exudate, tenderness, fixation, the position, size, tenderness and fixation of the uterus and appendages.

When one considers that the acute symptoms in salpingitis are caused by the escape of infective material from the abdominal ostia of the tubes, one can readily understand how a bimanual examination may be dangerous. I have known it bring discredit to the medical attendant.

Salpingitis is almost invariably bilateral, while an ectopic gestation or ovarian cyst is more evident on one or other side.

As modern teaching recommends that the treatment of acute salpingitis should be expectant (I have elsewhere expressed my disagreement with this[1]) it becomes of supreme importance to distinguish between acute salpingitis and ruptured ectopic gestation or other condition in which the need for urgent operation is unquestionable.

In case of any doubt a posterior vaginal cœliotomy should be done. This will at once disclose the true condition; if it be ectopic gestation, abdominal section and removal of the gravid tube are carried out straight away. If, on the other hand, pus is found, this is drained with a split rubber tube, constituting the first stage of my two-stage method of treating pelvic suppuration, to be followed in one or two weeks at longest by abdominal section and removal of the pus focus or foci.

Chronic Salpingitis.

The chief points in the diagnosis of chronic salpingitis are: (i) The history of the case, (ii) the exacerbations, (iii) the bilateral tender masses in the postero-lateral fornices, (iv) the pus which can almost always be expressed from Skene's glands and is visible through the speculum in the *os uteri*.

Although the ovary is so generally involved in the tubal inflammation that many authors refer to the condition as "oophoro-salpingitis," it cannot be too strongly emphasized that this involvement is usually merely a cortical inflammation which quickly subsides on carefully removing the tubes, leaving the ovaries to carry on their marvellously important functions in the economy. It is absolutely unjustifiable to remove the ovaries except in very rare instances; yet such ruthless mutilation is common in my experience.

On one occasion a husband offered myself and another gynæcologist £1,000 to help him to take action against a surgeon who had removed his young wife's ovaries. The exceptions to the law that ovaries should be preserved are: (i) Puerperal streptococci infections which *vid* the lymphatics may lead to ovarian abscesses even without affecting the tubes to any great extent, (ii) tubo-ovarian abscesses, generally the result of long continued infection with pus ulcerating through the wall of the tube into an ovarian follicle.

The following interesting case history of a patient now convalescent exemplifies many of the points raised in the foregoing remarks.

A.B., *ætatis* twenty-three, was admitted to hospital at midnight on December 19 with the history that she had been suddenly seized with pain all over the abdomen at 1 p.m. that day. The pain had increased and there had been vomiting. Menstruation had appeared at the proper time the day before the onset of the pain. There had been coitus and an abortion had been induced a year previously from which there had been a slow but eventually complete recovery. She had never before experienced a similar attack and usually enjoyed good health. On admission

[1] *Surgery, Gynecology and Obstetrics*, February, 1925, page 175.

the temperature was 38·3° C. (101° F.), the pulse rate 114, respiration 20. A hypodermic injection of morphine was given. I saw her at 10 a.m. the following day and noted the following. The face was flushed and anxious, the tongue furred and dry, the temperature was 37·8° C. (100° F.), the pulse rate 108. The patient lay on her back, with the left thigh flexed, which position she said gave her slight relief. The abdomen was absolutely flat, but extremely tender and rigid all over, more so below than above the umbilicus.

On examination menstrual blood was found at the vulva. On careful cleansing, no marked redness of the vulva could be detected, but pus was expressed from Skene's glands. The vaginal vault with the uterus was quite fixed and extremely tender, exudation was well marked so that the appendages were not defined. The uterus appeared to be of normal size and the os was closed.

On this history and these facts I based a diagnosis of perimetro-salpingitis and, as the patient said she was much better, the temperature and pulse were lower and vomiting had ceased, I was tempted to depart from my usual practice and postponed operation to allow of the further improvement which appeared probable.

On December 21, at 10 a.m., the temperature was 37·8° C. (100° F.) and the pulse rate 116. At midnight there was an exacerbation of the pain, the vomiting returned, the temperature increased to 38·9° C. (102° F.) and the pulse rate to 120. The abdomen was greatly distended. On December 22, at 10 a.m., forty-eight hours after I had first seen her, the temperature was 38·9° C. (102° F.), the pulse rate was 128 and weak. An enema failed to move the bowels or relieve the abdominal distension. Under ether anæsthesia, after the injection of two drachms of iodine into the uterus, the posterior vaginal fornix was incised and dark blood tinged with pus was evacuated. The abdomen was then opened, disclosing acute peritonitis of all parts within view, intense redness, plaques of recent lymph all over the small intestines, recent soft adhesions and a large quantity of thin, blood-stained fluid. Both tubes were enlarged and inflamed, the fimbriated ends were not closed, but the fimbria were much swollen. Both tubes were carefully dissected off the ovaries and removed. As the right ovary was slightly enlarged and soft, a narrow-bladed knife was passed into its centre and then rotated on its axis to make certain that no pus was present. The appendix was in the north-west position and could not be easily removed through the median incision, owing to congenital fixation of the caecum. It appeared to be unchanged and was therefore allowed to remain.

A narrow strip of iodoform gauze on the stretch was passed into the vagina and the abdomen closed. Although the operation had lasted only thirty minutes, the pulse was scarcely countable at its close. The head of the bed was raised on blocks to facilitate drainage, saline solution and glucose rectal injection were given and a submammary injection of saline solution. These measures, with frequent doses of 0·01 gramme (a sixth of a grain) of morphine, brought about speedy improvement, so that in six hours the pulse was of fair volume, the rate being 114, and the temperature was 38·35° C. (101° F.).

The next day (December 23) the pulse rate ranged from 112 to 122, while the abdomen was still much distended.

On December 24 the bowels had been slightly moved and flatus expelled by turpentine enema preceded by eserine and pituitrin. The distension had lessened and the expression had become more confident. The temperature was 37·58° C. (99·6° F.) and the pulse rate 96. The gauze in Douglas's pouch was pulled down half an inch each day in order to insure that no discharge was pent up. Thereafter convalescence progressed satisfactorily.

Dr. Shearman kindly reported on the pathological specimens as follows: The larger tube contained large blood clot, but no trophoblastic tissue. Doubtful Gram-negative diplococci pus cells were present. Culture showed staphylococci. In the smaller tube there were pus cells.

I realize the case has not been completely unravelled. The gravity of the condition, the extent and acuteness of the peritonitis excludes gonococcal perimetro-salpingitis as usually seen. Except for the blood clot in the tube and the small quantity of blood in Douglas's pouch there is no resemblance to ectopic gestation; indeed, the absence of chorionic villi shuts out this. I am therefore driven to the conclusion that the rupture of capillaries in the tubes, intensely engorged from the virulence of the infection, caused the hæmorrhage and that possibly the girl, imagining herself pregnant, paid another visit to the abortionist who infected the uterus with staphylococci.

Failure to follow my usual practice in such cases of opening into Douglas's pouch through a vaginal cœliotomy incision without any delay nearly cost the patient her life. Forty-eight hours later, when I did operate, the condition of the patient had greatly changed for the worse and the "blown-up" abdomen from paralytic ilius rendered the necessary procedures to remove the foci much more difficult and dangerous.

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British Medical Association News.

SCIENTIFIC.

A MEETING OF THE NEW SOUTH WALES BRANCH OF THE BRITISH MEDICAL ASSOCIATION was held at the Robert H. Todd Assembly Hall, British Medical Association House, 135, Macquarie Street, Sydney, on October 30, 1930, DR. E. M. HUMPHERY, the President, in the chair.

Pulmonary Disease.

DR. NEVILLE DAVIS read a paper entitled: "Some Recent Advances in the Treatment of Pulmonary Disease" (see page 133).

Radiography and Pregnancy.

DR. H. R. SEAR read a paper entitled: "Some Notes on the Value of X Rays in Pregnancy" (see page 137).

DR. COTTER HARVEY thanked Dr. Davis for his interesting *résumé* of his treatment of pulmonary diseases. He thought it was a pity that in Australia they had to accept artificial pneumothorax as a recent advance in treatment. In other parts of the world it had been used for twenty years. There was no doubt that they were at least a decade behind the times in Australia in some aspects of the treatment of pulmonary disease. Artificial pneumothorax should be contemplated by all medical practitioners as a possible form of treatment in bronchiectasis, pulmonary abscess and pulmonary tuberculosis. There was one possible exception he would make to the indications given by Dr. Davis. In acute pulmonary tuberculosis the expediency of waiting twelve weeks might be considered, but in the presence of an acute progressive unilateral lesion it was in Dr. Harvey's opinion inadvisable to wait at all. In regard to tuberculin, he thought that they were all agreed that it could be consigned to the limbo of the past—it had been weighed in the balance and found wanting. "Sanocrysin" had come to stay. He had had experience of it for three years and had used it in the treatment of some twenty patients. He was satisfied as to its value and hoped to make a communication on the subject at a later date. In regard to artificial pneumothorax as treatment for bronchiectasis, it sounded quite simple, but it was not always easy to get good results. Fibrosis and thickening of the wall sometimes made it difficult to cause collapse of a bronchiectatic cavity. He thought that they had to confess that recent advances in the treatment of pulmonary disease were almost entirely surgical. He had done some work along these lines with Dr. Coppleson and he hoped that the latter would say something on the subject. He believed that phrenic avulsion and thoracoplasty had come to stay; the literature on the subject was increasing, criticism was diminishing and a standard treatment was gradually being evolved. It was, he thought, a pity that they had not reached the stage of invoking the aid of the surgeon in the treatment of some of their patients.

In conclusion Dr. Harvey showed films taken from several patients. He demonstrated the condition before treatment in one instance and showed that as a result of "Sanocrysin" treatment an exudative type of lesion had been replaced by one of the fibrotic type. In another instance he had sent a patient back to her medical attendant after satisfactory sanatorium treatment. The practitioner had thought to round the treatment off by giving a course of tuberculin. Dr. Harvey was not prepared to state whether what followed was *post hoc* or *propter hoc*, but there had been a general recrudescence of symptoms with reappearance of sputum. X ray examination had then revealed a diffuse exudative condition of the base of the left lung. This had yielded in a remarkable fashion to the administration of "Sanocrysin."

DR. V. M. COPPLESON expressed his thanks to both authors. He had read accounts of the effect of X rays on growing tissues and he would like to hear Dr. Sear's view of the effect of X rays on the fœtus. Although not an obstetrician, he was interested in a case in which the patient had suffered from a fracture of the pelvis, the head of the femur having been driven through the acetabulum. Later on the woman had become pregnant. Skiagrams, as Dr. A. J. Gibson would be able to agree, for he had looked after the patient, had been of considerable assistance in determining the alteration in the shape of the pelvis.

In regard to chest surgery, he had had the good fortune to be associated with Dr. Harvey and Dr. Stobo at the Pulmonary Clinic of the Royal North Shore Hospital of Sydney. There was no doubt that X rays had opened up new avenues and given a new outlook in diseases of the chest. It was possible to determine the extent of the lesion and by the use of "Lipiodol" they were able to make accurate diagnosis where previously this had been almost impossible. Surgical measures in pulmonary tuberculosis could be undertaken only when the condition was limited to one lung. In regard to artificial pneumothorax, empyema occurring as a sequela was most serious. The infection gave rise to a pyopneumothorax which caused the lung to collapse; this was much more serious than when fluid only was present which was merely lifting the lung. He thought that the question might be raised as to whether phrenic avulsion should not be performed as a routine measure at the conclusion of artificial pneumothorax treatment. The operation of phrenicotomy was not at all a difficult procedure. Its value was doubtful in bronchiectasis and abscess of the lung.

DR. H. C. E. DONOVAN expressed his appreciation of the papers and said that he was glad Dr. Sear had raised the subject of the use of X rays in pregnancy. Probably X rays were not used as much as they could be. There were many difficulties and some dangers. As far as actual measurement was concerned, in computing the proportion between the fœtal skull and pelvis, it was surely a matter of great difficulty to obtain mathematical certainty. The rays were divergent and the three factors concerned were placed at different distances; there were, in other words, differences of distance between the source of the rays and the target. It would probably be possible to obtain a computation, but he doubted whether the degree of accuracy obtained was equal to that obtainable by palpation and the use of the fœtal head as a pelvimeter. There was a certain amount of evidence that X rays were dangerous—when used for therapeutic purposes they were certainly dangerous and when they were used photographically danger probably existed. In 1928 Murphy, writing in *Surgery, Gynecology and Obstetrics*, had reviewed 328 human pregnancies in which X rays had been used and had applied X rays to a considerable number of pregnant animals. He had found that when considerable irradiation was used there was danger of the offspring being affected. In 61% the offspring had been defective. Murphy had concluded that it was not justifiable to use X rays unless it was intended that the pregnancy should be terminated artificially. In 1930, writing with Goldstein in the *American Journal of Roentgenology*, Murphy had concluded that 24% of 650 pregnancies following preconceptional or postconceptional irradiation, had ended in abortion and that 13% terminated in the birth of unhealthy children. Among forty-six cases in which preconception irradiation had been employed, twenty-four abnormalities had been explained by disease of the mother and twenty-two had been explainable only by the previous irradiation. Dr. Donovan concluded that X rays should be used in pregnancy for carefully selected patients only and with a full knowledge of the dangers involved. Radiological examination was of undoubted value in fat women in whom it was difficult to distinguish between a mid-term pregnancy and a fibroid tumour of the uterus.

The same precautions were necessary in regard to the use of "Lipiodol." It was a valuable advance, but carried dangers if used carelessly, especially if care were not taken to exclude infection of the *cervix uteri*.

In the present state of knowledge it was better to carry out a very careful antenatal examination than to use X rays as a routine measure for diagnosis.

DR. A. J. GIBSON, after thanking both speakers, said that he was interested to learn that it was possible to make a radiological diagnosis of pregnancy at the eighth or ninth week. This would be very useful in certain special circumstances. Ordinarily X rays were not needed. They would be useful in irregular menstruation in the presence of a rapidly growing uterus, to determine whether pregnancy or a fibroid tumour existed. Dr. Gibson quoted the history of a patient which demonstrated the practical advantages gained from radiological examination in determining whether operation should be undertaken. In regard to many of the malformations or still-births referred to by Dr. Donovan, X rays had been used for treatment and not merely for diagnosis. He agreed that there was definite evidence of ill effects if pregnancy occurred too soon after radiation treatment. One X ray exposure for photographic purposes was not followed by trouble to the fœtus. Dr. Gibson referred to several cases illustrating these points. It was stated by some observers that Cæsarean section should not be done until X ray examination had revealed a normal fœtus. While he doubted the advisability of accepting this statement, he did think that X rays were useful in the presence of an over-distended uterus to reveal the presence of either a hydrocephalus or a monster.

In the diagnosis of pelvic measurements he was interested to hear the claims that had been made, but he pointed out that the pelvis was only one factor and that it was the size of the fœtal head which counted. In this, as in all matters, it was necessary to use judgement. There was scope for the use of X rays, but they should be used only when it was impossible to come to a satisfactory conclusion by clinical means.

DR. A. T. NISBET referred in terms of appreciation to the two papers and said that many of the difficulties in regard to radiological examination during pregnancy arose from failure on the part of medical practitioners to realize the difference between hard and soft rays. The patients quoted by Murphy had been irradiated for certain diseases and the bad effects had obviously been the result of hard rays. He had taken skiagrams of about fifty pregnant women and had received no complaints in regard to dead or malformed children; moreover, he had not used particularly soft rays. In regard to the measurement of the inlet and outlet of the pelvis and the size of the head, they had to remember that the Potter-Bucky diaphragm cut off divergent rays and determined more accurately the relationship of the parts.

DR. A. J. HOOD STOBO thanked both speakers and said that the setback to the use of artificial pneumothorax in Australia was due to the fact that it was regarded by many as a specific treatment for pulmonary tuberculosis. As a consequence there was a tendency to neglect such general measures as the insistence on rest and fresh air and the giving of good food. In the United Kingdom and on the Continent of Europe artificial pneumothorax was generally used in sanatoria where these general measures were adopted and as a consequence better results obtained. Dr. Stobo concluded by pointing out that the main treatment in bronchiectasis, even though an artificial pneumothorax had been induced, was postural draining and that this was often forgotten.

DR. H. A. RIDLER said that X rays were of little use in the diagnosis of contraction of the pelvis, as the diagnosis of contracted pelvis was not sufficient by itself as a guide

to the method of delivery. He had used them more in order to determine whether the presentation was a frank breech or not. X ray examination was also useful in the diagnosis of anencephaly. Dr. Ridler quoted cases illustrating these points.

DR. B. P. ANDERSON-STUART referred to the question of the danger of X ray examination as far as the fœtus was concerned. It was quite clear that heavy therapeutic doses would produce fœtal injury. In fact, at Mount Sinai Hospital, New York, he had found that an intensive dose of radiations was used whenever it was necessary to produce an abortion. He had made approximately sixty diagnostic examinations of the pregnant uterus and had received no complaints. There had been no fatalities except one. In this instance the fœtus, weighing four and a quarter pounds, had been very weak and the death had obviously been quite unconnected with the use of X rays. He could not conceive that injury to the fœtus was possible in X ray diagnosis with the rapid films at present in use.

Dr. Davis in reply to Dr. Harvey referred to the question of waiting in acute pulmonary tuberculosis before performing artificial pneumothorax. He agreed that it was well to wait eight to twelve weeks in ordinary tuberculosis. He was at one with Dr. Harvey in the view that no delay was justified in the presence of rapidly progressing disease. He also referred to the value of albumin found in the sputum. It was often present when no tubercle bacilli could be discovered. Artificial pneumothorax should be carried out early in restful and hygienic surroundings, such as were provided by sanatoria. As far as gold salts were concerned, he had used them in eighteen cases in the form of "Solganol B." He had found it most advantageous in bilateral disease, either when artificial pneumothorax had not been beneficial, or when it had only produced partial result and refills were no longer possible. He had probably been misunderstood in regard to bronchiectasis. He had not intended to convey the impression that he regarded artificial pneumothorax as a satisfactory therapeutic measure in late bronchiectasis when fibrosis of the surrounding lung tissue had occurred. He had referred to the early stages of the condition when the bronchi were dilated, sputum was abundant and fibrosis had not resulted. His experience with thoracoplasty was limited to two cases. In these instances both patients had seemed better, but the operations had been very extensive and the patients had not appreciated what was done for them. In regard to empyema following artificial pneumothorax, he had had experience of only one case. In this instance he had pushed the exploring needle into an infected cavity in the lung—the condition had been the result of faulty technique. Empyema was a rare condition after artificial pneumothorax. As far as surgery of the phrenic nerve was concerned, he believed that if an artificial pneumothorax was done efficiently and was kept up long enough, the lung would heal and arrest occur. If diaphragmatic adhesions were present, so that it was impossible to produce a satisfactory pneumothorax, some operation on the phrenic nerve should be undertaken.

In his reply Dr. Sear said that Dr. Donovan had misunderstood him. He had referred only to measurements of the pelvis and had not intended to make any reference to the fœtal head. As far as the danger of X rays in pregnancy was concerned, he thought that the question had been already answered by Dr. Nisbet and Dr. Anderson-Stuart. If they cut out X ray examination in pregnancy they would be doing away with a very useful measure. There was absolutely no danger to the fœtus with one or two diagnostic exposures.

NOMINATIONS AND ELECTIONS.

THE undermentioned has been nominated for election as a member of the New South Wales Branch of the British Medical Association:

Gordon, Charles Patrick, M.B., B.S., 1930 (Univ. Sydney), Sydney Hospital, Sydney.

The undermentioned have been elected members of the New South Wales Branch of the British Medical Association:

Anderson, Douglas Joseph, M.B., B.S., 1930 (Univ. Sydney), Sydney Hospital, Sydney.

Franklin, Samuel de Vere, M.B., Ch.M., 1921 (Univ. Sydney), Goodooga.

Gaffney, Thomas Joseph, M.B., 1930 (Univ. Sydney), Bonalbo.

Philpott, Jack Melville Curran, M.B., B.S., 1930 (Univ. Melbourne), Goulburn District Hospital, Goulburn.

Read, John Richard Major, M.B., Ch.B., 1929 (Edinburgh), 8, Porchester Hall, Bellevue Road, Bellevue Hill.

Ross, Angela Mary, M.B., 1928 (Univ. Sydney), Hill Street, Lithgow.

Medico-Legal.

MACKELLAR *v.* ALLPORT.

IN this action the plaintiff claimed £2,500 for alleged unskilful and negligent treatment by the defendant of certain injuries to the plaintiff's leg and foot. The action was heard in the Supreme Court, Sydney, before Mr. Justice Halse Rogers and a jury of four on December 8, 9, 10, 11, 12, 15 and 16, 1930.

Mr. Manning, K.C., and Mr. Reimer appeared for the plaintiff.

Mr. Feez, K.C., and Mr. Alroy Cohen appeared for the defendant.

PLAINTIFF, sworn, examined, deposed:

To Mr. Manning: My name is John Athol Mackellar. I carry on a small grazing area at Coolah of 1,024 acres. On September 3, 1928, my sheep were being shorn at the Pine Ridge wool-shed by contract. After the first day's shearing I went out to get some more sheep into the shed. I opened the doors and filled the pen up with sheep. I closed the doors and as I was walking away the big door fell out of its socket and fell on my back and knocked me over; it fell on the calf of my leg and made an abrasion there. It knocked me down and my leg was broken. Mr. Snell and the contractor, who were around in another part of the shed branding sheep, came round and they found me under this door. The pain was pretty great, so they packed me into a car and took me to Dunedoo to Dr. Evans. I was carried into the surgery by Mr. Snell and the contractor. Dr. Evans examined me, shaved my leg and put sticking-plaster all round the fibula, which he said was broken. Then he told Mr. Snell to put me into bed for the night. I was taken to the hotel and they put me into bed for the night at the hotel. Dr. Evans advised me to go down to Dr. Allport and he would get in touch with Dr. Allport and arrange for a bed for me. The following day Mr. Snell called for me and I was taken by car from Dunedoo to Gulgong Hospital. When I got to the hospital at Gulgong I did not see Dr. Allport at the hospital. I was carried into bed by Mr. Snell. Then I was taken up to Dr. Allport's by Mr. Snell in the car. When I got to the surgery I was carried in by Mr. Snell. I saw the doctor. I was put on to the table. He put my leg up and looked at it. Then he said he would take an X ray. He asked me where the pain was and I told him it was where the fibula was broken and I told him the ankle. It was very painful and terrifically swollen at the time. At one time when the X ray was being taken I made the remark that the ankle was such a terrific looking thing that it would never get better. The doctor just took the X ray. He turned my foot over and took it from the lateral view and then looking straight down on to the foot. Having taken it he went out of the room and came back in about ten minutes' time. I was suffering very great pain at the time. He held the X ray up to a window and he said there was nothing wrong with my ankle but that my fibula was broken. I remarked that it was jolly pain-

ful. He did not say anything more. I was taken back to the hospital by Mr. Snell and I was put into bed. I should say the doctor came down twenty minutes to half an hour after I was put into bed. The leg was very painful then and very swollen. The ankle was all blue. The doctor came out with the matron and a box-splint. The doctor said at the time that there was nothing wrong with my ankle but that the fibula was broken. He then proceeded to put it up into this box-splint. The box-splint was lying flat on the bed. I was in pretty bad pain. I just held the top rails of the bed tightly and he lifted my leg up and put it into the box-splint, padded it where the fibula was broken and also under the leg and heel. Then he pushed the foot-piece up, which is attached to the back splint, until it came in contact with the foot. Upon coming in contact with the foot it was lying over to the side that way, so he pushed it in line with the foot, which caused a considerable amount of pain. Then he gradually pushed the box-splint up till he got it in perfect position with the foot, then he bound the foot to the upright, then he gradually pushed it up towards me. When he was doing that, the pain was so great that I practically fainted.

That is all that was done that day.

The doctor came down later that afternoon, but he did not tell me anything; he just asked me how I was getting on. During all that day the leg was very painful. That night they gave me morphia because I could not sleep. The next day when the doctor came down I asked him was he sure there was nothing wrong with my ankle, as it was very painful. He said "no," the fibula was broken but the only thing wrong with the ankle was that the muscles and sinews had been torn. That is all he said then. It was very painful then. I asked him on that account to make sure that there was nothing wrong with my ankle. After that he went away. I remained in the splints for eight days until about September 12. During that period of eight days I suffered pain. About a week after I was in the splint my leg was particularly sore. The doctor had given me the X ray during that period and on this particular day when he came down I asked him would he have a look at the X ray to make certain there was nothing wrong with my ankle, as it was very painful. He asked me had I the X ray. I said "yes," it was in my locker. I gave him the ray. He held it up to the light and he said: "The fibula is broken, but there is nothing wrong with the ankle except, as I said before, the sinews and muscles have been torn."

(X ray photograph tendered and marked Exhibit "A.")

The leg remained in splints until September 12. On September 12 the doctor came down and took my leg out of the splint and proceeded to put it into plaster with the aid of the matron. When he took it out of the splint, although the leg had gone down the ankle was all out of shape and swollen. The doctor did not say anything about that, I pointed that out to him. I remained in plaster for about two weeks. That took me up to about September 26. During the period from September 12 until September 26 my leg was in the plaster. After about two days I think I was allowed out of bed on crutches. That was during that period. Originally when he put it in the box-splint nothing was done by him to the ankle at all; it was never manipulated in any way whatsoever; it was just placed in the box-splint as I have described it. The pain still continued. During the time it was in plaster I did not make any complaint to the doctor about pain. I told him at the time he put it in that it was sore, but during the two weeks I did not complain. On the 26th the plaster was taken off. There was no pain when he was taking it off, but afterwards when I got out of bed there was pain. When I got out of bed I think the doctor had gone. I saw him the next day. When he came the next day he told me to exercise it. It was bound round with just a light bandage. I used to try to put it on the ground, but it was so painful that I could not put it on the ground, I told him. The doctor told me it was only the sinews that were torn and it would get better. During the whole time I was in the hospital after the plaster was taken off, if I tried to put it on the ground at any period it was always swollen and sore. That continued until the day I left. I saw the doctor on October 7, the day I left the hospital. He was trying to get me to walk without any crutches at all. I tried and I said: "It is impossible, Doctor; I cannot walk at all." He told me I only needed exercise. I tried, but I could not do it. I said to the doctor: "Can you do any more for me, Doctor?" He said: "No, you only need exercise." I said: "Can I go?" He said: "You can go if you like; you want to get off the crutches as soon as you can." I went to Wentworth's (friends at Coolah) and stopped there for about ten days. During that period I tried to walk as the doctor instructed me, but it was so painful that I could not do anything with it at all. After I left Wentworth's I did not see Dr. Allport. I decided to see a doctor and I went straight to Sydney. I was taken down by car to see a doctor. I came down by car to Sydney and went to stop at my father's place. While there I saw Dr. Stevenson. I showed my leg to Dr. Stevenson and he advised me to have an X ray taken. I then had my leg X rayed at the Mater Misericordiæ Hospital. After I got that X ray I went back to Gulgong. I was on my way back home then. My leg was in plaster. It had been put in plaster by Dr. Stevenson. On my way home I called in and saw Dr. Allport at his surgery. The first thing I said to him was: "Well, Doctor, my ankle was broken." He asked me if I had an X ray showing it. I said "yes." He asked me would I mind if he had a look at it. I said "no." I handed him the Mater Misericordiæ Hospital X ray. He studied it and asked me where I thought the fracture was. I showed him and he said: "That would not cause any trouble at all." Then he took down a lot of books from his shelf. He asked me if I would show him where I thought the fracture was and I showed him what I thought was the fracture. Then he took down a lot of books to show me why this fracture would not cause any trouble. That was the tibia. I said: "We have had a lot of difference over the X ray" and I suggested he should have it sent down to an expert in Sydney to have it read. I said I would pay the cost of the reading if he would name the man. He just said he did not know any. I went away then. I asked him would he take the plaster off when it was to come off. I told him also that the doctor had told me that there was a break in the fibula, that the union in the fibula was not strong and that there was a fracture in the ankle. I think I saw Dr. Allport about two weeks after I got home, when I went back to his surgery again to get the plaster taken off. He got the plaster off, then he got me off the table and had a look at it and he said: "I will take another X ray now," and he took another X ray. That was after he had got the plaster right off and after I had complained about the pain. He took another X ray and said he would send it on to me. I received the X ray later on with a letter. When Dr. Allport was taking the plaster off I told him that Dr. Stevenson had suggested that I should have massage, and he said it was a very good idea. He asked me if I knew any masseur and I told him in Dunedoo there was one, so he wrote to Mrs. Cleary and arranged for me to go there. After I left Dr. Allport's surgery I was still walking on crutches. I could not put my foot on the ground. I arrived at Mrs. Cleary's on crutches. The foot was all misshapen, it was all blue and it would not move at all; there was practically no movement at all. Mrs. Cleary took me in charge and she used to do the ordinary massage, starting from the hip down for about a quarter of an hour, then she began to work the foot slowly at first in little movements, both forward and lateral movements. The slightest movement at that time caused me a lot of pain. As time went on she moved it more. It was most painful and at times I used to weep with the pain. She said it had to be done, as she wanted to stretch the muscles and get them all back into their normal position. She used to try to get me to walk, but only the last few days I could walk a few steps. When I had my last massage on the 19th December the leg was not in a good state at all; it was in a very bad state, and I decided to see the specialist. I was driven down to Sydney. On my way down I called in to see Dr. Allport and I told him that I was not satisfied with my ankle and that I intended to come down and see Dr. Royle. He asked me would I like a letter; I said yes, so he gave me a letter to Dr. Royle. When I was leaving to go down to Sydney

Dr. Allport did not make any remark about my condition. He just gave me the letter. I came down to Sydney then and consulted Dr. Royle. After that I stopped down and I went and saw Dr. Hertz. I showed him Dr. Allport's original X ray, the Mater Misericordiæ Hospital's X ray, Dr. Allport's second X ray and an X ray taken by Dr. Oxenham which Dr. Royle had asked me to have taken. Dr. Hertz examined my foot carefully and gave me certain advice. The next doctor I saw was Dr. Corlette; that was somewhere about the 20th or 23rd January, 1929. After I saw Dr. Corlette I went back to Coolah. I think I saw Dr. Allport on my way back after I had seen Dr. Royle. I had Dr. Oxenham's X ray then and I pointed out to Dr. Allport the place on the leg where Dr. Royle said there was some trouble. When I pointed that out to him on Dr. Oxenham's X ray Dr. Allport did not say anything. Later on in the year after the writ had been issued, my solicitor got a letter from Dr. Allport's solicitor and I was asked to submit my leg to an examination. They named the doctors to whom I was to go; they were Dr. Corlette and Dr. Sheldon. I submitted to an examination. I also submitted to an X ray being taken by Dr. Edwards. After I had seen Dr. Corlette and Dr. Sheldon I saw Dr. Benjafield in, I think, December. Dr. Benjafield made a complete examination of the leg and measured all the leg. Last week I was asked by the defendant to submit to a further examination, and I was examined by Sir Alexander MacCormick and Dr. Corlette.

EMILY FRANCES WENTWORTH, sworn, examined, deposed:

To Mr. Manning: I am the wife of Mr. George Wentworth who has a station property at Glencoe, Coolah. I know the plaintiff. When he first came to us his leg was very swollen and very ugly looking. It was purple. It looked bad—peculiar. It was purply, queer-looking, unhealthy. The swelling was mostly down about the ankle. When he was staying with us he certainly did show signs of being in pain. He was always in pain when he tried to use it at all. He said that the doctor said he was to exercise it and he tried to exercise it. When he tried to exercise it, he simply could not, it was too painful. I bathed it with hot water and put a small bandage on, and then it used to swell over the bandage and two or three times a day I would have to take the bandage off because it would be swollen over the bandage, and bathe it again and then bandage it again. I always did it at least twice a day. I did not see any sign of improvement in the leg when he was with me.

PHILIP SIDNEY PARKINSON, sworn, examined, deposed:

To Mr. Manning: I am a radiologist. I remember the X rays taken of the leg of Mr. Mackellar. I look at Ex. A. I have seen that X ray before.

Q.: I want you to indicate to the jury all the indications you see there of any injury to the leg or other part. *A.*: I can see a fracture of the middle third of the fibula, a fracture of the posterior aspect of the lower end of the tibia, a fracture of the posterior aspect of the tibia and an interruption of the line of the articular surface.

Q.: As regards displacement of the astragalus, what do you see there? *A.*: The astragalus is displaced slightly backwards.

Q.: Did you take a ray yourself of the normal leg, and did you make a comparison between Ex. A and the ray of the normal leg which you took? *A.*: Yes.

Q.: Look at that photograph (handed to witness) and see if it is the photograph of the normal leg which you took? *A.*: Yes.

Q.: Was your reading further confirmed on a comparison of that with Ex. A? *A.*: Yes, it is taken in a slightly different position.

Q.: On examination of Ex. B, which is the Mater Misericordiæ ray, what do you see there in the way of indication of injury? *A.*: You see the same indication of injury. You see the overlapping of these two fragments here where an extra thickness of bone gives you greater capacity.

Q.: And the displacement at the astragalus? *A.*: That is much the same, though it is perhaps a little more obvious.

Q.: Would you look at Ex. C? That is the second one taken by Dr. Allport. What indications of injury do you see there? *A.*: You see the same indications.

GERTRUDE MARY CLEARY, sworn, examined, deposed:

To Mr. Manning: In November, 1928, I was living at Dunedoo. Some time in November, 1928, Mr. Mackellar, the plaintiff, came to me for massage treatment. Mr. Mackellar's leg was very swollen when he came to me. It was in a bluish-red condition and the muscles and tissues were very hard. He was suffering pain. I proceeded to apply massage treatment. I massaged the whole of the leg. I do not remember how long that treatment continued. During the time I was massaging him I saw him not quite every day, but nearly every day. When he first came to me he was on two crutches. During the course of my massage the condition of his leg improved. I suggested to him that he should exercise it, and I tried to get him to exercise it. During the treatment a lot of the swelling went down and the tissues got very much softer. I got him on to one crutch and finally on to a stick. Once or twice he endeavoured to walk without the stick at all. It was not a very good walk. There was not perhaps the correct spring in the foot. He was able to sort of limp a little bit. He was on the one stick when he left me. During the course of the massage treatment there were indications of his suffering pain when he tried to walk.

VIVIAN BENJAFIELD, sworn, examined, deposed:

To Mr. Manning: I am a legally qualified medical practitioner. I remember in December, 1929, making an examination of Mr. Mackellar. I found on examination that there was backward displacement of the foot at the ankle joint. One could tell that, because the front of the bones of the leg as distinct from those of the foot were more prominent than in the other foot. There was some general increase of the size of the ankle joint. That is what we usually find after such a fracture. There was no swelling or undue redness. Dorsiflexion was limited to 95° as compared with 85° on the other side. Plantar flexion was 125° as compared with 150° on the other side. That was by actual measurement with a rule and a protractor. I diagnosed the case as a fracture with posterior displacement of the foot.

Q.: Will you tell His Honour and the jury what is the proper remedy to apply to a patient who comes in to you suffering from an injury of that kind? *A.*: At first, if it is possible one has an X ray taken to find the exact condition. The next procedure when your diagnosis is confirmed by the X ray is, preferably under an anæsthetic, to reduce the fracture and the dislocation. One reduces those at the same time. You pull the foot into position, pull it forward, holding the leg, and at the same time try and manipulate the broken fragment into place. Then, having done that, you put it up in retentive apparatus, preferably plaster, to hold it there.

Q.: What would you say as to treatment of this kind: without reducing it at all, putting it in a box-splint, and leaving it in a box-splint for eight days? *A.*: I think that would be ineffective.

Q.: Will you tell the jury why you think that would be ineffective? *A.*: I take it that putting it into a box-splint would push the foot down against the foot piece and fasten it there and fasten the leg down on to the splint. That would not bring the dislocated bone forwards—the talus—and pushing it to a right angle would tend to push the broken fragment upwards and so, if anything, make the condition worse.

Q.: You told us you made an investigation yourself of the joint and so on? *A.*: Yes.

Q.: You also saw the X rays? *A.*: Yes.

Q.: After you saw those, did you form an opinion as to whether with proper treatment that ankle and fracture should or should not have come back to normal? *A.*: I formed the opinion that the fracture and the dislocation or posterior displacement was still in existence, that is, that the fracture had not been reduced, although it had healed, and that the talus, the astragalus, was still bad.

Q.: But if the dislocation had been reduced and the proper treatment applied, was there a reasonable prospect, in your opinion, of the leg coming back to normal and normal use being restored? *A.*: Yes.

MAX HERTZ, sworn, examined, deposed:

To Mr. Manning: I am a legally qualified medical practitioner and I carry on my profession in Phillip Street, Sydney. I remember making an examination of the plaintiff's leg on 9th January, 1929. I examined Mr. Mackellar's right foot and afterwards made an examination of the X rays which he brought with him. On the first examination apart from the X rays I found that the foot was deformed—thickened. He had a limitation in movement and the outline of the foot was not normal, especially in front. The fore part of the tibia was jutting forward—protruding. The ankle was thickened, swollen, and there was a limitation in movement. I formed the conclusion that there was a fracture of the tibia and fibula with posterior dislocation of the foot.

Q.: Would it be proper treatment to put that limb in a box-splint and leave it there for eight days and take it out again without having reduced the dislocation? *A.*: No.

Q.: Would there be any justification medically for such a course? *A.*: The dislocation must be removed. That is essential. If not, we get this deformity, which is permanent.

Q.: As regards the possibility of treatment, when the patient came to see you did you then consider that anything could be done to remedy the harm that was done? *A.*: It would be very difficult and very doubtful.

Q.: Would you have recommended an operation or anything of that kind, to see what could be done? *A.*: I would not recommend it.

Q.: I want you to look at Ex. B, but just before you come to that particular one, at the examination that you made did you form any opinion from what you saw there as to the manner in which the foot had been set? *A.*: One could only see that the dislocation had not been reduced, had not been removed.

Q.: And that being so, could you give any opinion as to the permanency or otherwise of the injury? *A.*: The injury would be permanent.

Q.: Permanent to what extent? *A.*: Limitation of movement, that is the greatest thing, of course, and soreness or pain.

DEFENDANT, sworn, examined, deposed:

To Mr. Feez: My name is Robert Murrell Allport. I am a legally qualified medical practitioner carrying on the practice of my profession at Gulgong. I saw the plaintiff in the hospital first. He was just being put to bed. I had a look at his leg and made a short examination of it and then had him transferred to my surgery for X ray treatment. When I first saw the leg it was very swollen. It looked quite a bad ankle. It was an extremely bad swelling. I had the plaintiff taken back to his car and he was taken up to the surgery to be X rayed. The plaintiff was complaining of pain principally in his back. He gave me a short history of the accident. Dr. Evans had given me a short history beforehand. The plaintiff said that his back was sore, that was all. He said that a door had fallen on him and had hit him on the back. I have an X ray apparatus. I do not practice as an expert; it is just to help me in my ordinary practice, to try and get more information than I can get without it. When the plaintiff arrived at my surgery he was placed on the couch and I made an examination of the ankle. I then found that the ankle was terribly swollen. From my clinical examination I discovered a fractured fibula. The ankle was enormously swollen; it was too swollen to find any position of the bones or anything like that. I then X rayed the foot. When the actual X ray was taken I think Mr. Snell was present. I developed the X ray at once. The plaintiff had been taken back to the hospital in the meantime. As far as I know Mr. Snell went with him. When I examined the X ray I found that the plaintiff had a comminuted fracture of the fibula in about the middle third and also he had a fracture of the posterior aspect of the tibia, that is, the posterior lip of the tibia. I examined the X ray for displacement but I could not find any. As far as the fracture of the tibia was concerned, I found it was displaced up very very slightly. I call that lipping. It was in excellent position. I look at Exhibit "A." I cannot now see any sign of displacement there.

Q.: Is there anything in that which leads you to the conclusion that there is not displacement? *A.*: Yes.

Q.: Will you just explain what it is? *A.* (examining X ray film over light): First of all, I find that this line running down is interrupted there and the articular surface is interrupted there. That piece is not to my mind pushed back; it is pushed straight up; it is in contact with its parent bone. In taking the position of the talus or the astragalus, the articular surface of the talus is in its proper position as regards the posterior aspect of the tibia. That conveys to my mind that the talus is not pushed back. If it had been pushed back, the tibia and fibula must have been separated. The tibia and fibula are in perfect position, in their normal position; they have not been interrupted at all.

Q.: Is there anything else in that X ray to lead you to think there was any backward displacement? *A.*: No.

Q.: You then came to the conclusion that the plaintiff was suffering from a fractured fibula and a fracture of the posterior portion of the tibia without displacement? *A.*: Yes.

Q.: What did you do after that? *A.*: I then went down to the hospital and arrangements were made to treat him. The plaintiff was lying in bed and in the presence of the matron I examined his leg. I flexed his knee. I examined his ankle and with one hand underneath the *tendo Achillis* and the other hand on the sole I gradually brought the foot up to a right angle. I did not cause him any extra pain in doing that, as far as I noticed. When I had finished he said: "Is that all to be done?" I said: "Yes." That indicated to my mind that the bones were in good position. I could not have done that without causing him excruciating pain if the bones had not been in good position; it would not have been possible. I developed the X ray plate and examined it. Having examined the plate I proceeded to the hospital and arranged to make a treatment. I put the plaintiff in a box-splint. I manipulated the foot itself. I had my hand underneath the *tendo Achillis* and then gradually brought the sole up to a right angle. There was no difficulty in doing that.

Q.: The plaintiff says that you first of all bound his foot to this piece of wood and that the footpiece was hanging back at this angle (indicating); you bound it to that, then you forced this piece of wood up like that and forced his foot into what he called perfect position. You say that such a thing did not take place? *A.*: No. The ankle was held by myself as I have described and then the splint was put in by the matron underneath and the ankle lowered down into its proper position and bound at a right angle to that upright.

Q.: Would it be possible to do what the plaintiff suggests? *A.*: I do not quite follow what the plaintiff says. You cannot do it with that splint.

Q.: He says that the end piece was at a sort of angle, that you bound the foot to it and then practically forced it up to a right angle? *A.*: You could not do it with that splint. It is an impossibility.

Q.: It has been suggested that you should have put it in plaster. What do you say to that? *A.*: I would be very sorry to have it done to myself with a leg swollen like that.

Q.: What is the usual practice in the hospitals and the places you have been with regard to first attention in a matter of this sort? *A.*: To put it up in a box-splint until the swelling goes down.

Q.: When you were manipulating the leg for the purpose of putting it into that box splint, what position was the leg in? *A.*: The knee was bent. Then manipulation took place at the ankle.

Q.: You had him lying on the bed with his knee bent like that (indicating)? *A.*: Yes.

Q.: Then you say that you held him underneath the foot, the matron held the splint and you put him in? *A.*: Yes.

Q.: Assuming there had been any displacement, what would be the result of what you did to the plaintiff? *A.*: Reducing the displaced parts. The manipulation of the ankle joint with one hand under the *tendo Achillis* and gradually bringing the leg up to a right angle would have the effect if there was any displacement of carrying forward that part into its normal position.

Q.: That night the plaintiff was suffering some pain? *A.*: Yes.

Q.: What was his complaint generally then? *A.*: He was complaining more of the pain in the back.

Q.: Was that a serious complaint about his back? *A.*: Yes, the back seemed to be his principal worry to a large extent.

Q.: I think he said that at night when you came down he complained of his ankle to you again and that you said there was nothing wrong with the ankle? *A.*: Not that night.

Q.: Did you ever at any time tell him that there was nothing wrong with his ankle? *A.*: No, I did not.

Q.: Did you ever tell him what was wrong with him? *A.*: Yes, I did.

Q.: When was that? *A.*: That was on the second day after I had taken the X ray plate to the hospital. I took the X ray down and I explained to him where the fracture of the tibia was. I said there was a piece chipped off the back of the tibia. He then asked me how about the ankle and I said that the ankle would be, I thought, all right. The ankle was swollen. As a matter of fact, I think it was a little bit more swollen than it was the day before. He was still complaining of pain in it.

Q.: Was there any other pain that morning? *A.*: He was still complaining of his back. After he had been in the box-splint for eight days I decided that the swelling was reduced enough to put him up in plaster.

Q.: Eventually how long after that did you remove the plaster? *A.*: Fourteen days.

Q.: What did you find when you took the plaster off? *A.*: That the swelling had mostly disappeared from the ankle. It was naturally a little bit stiff. He was told to rest.

Q.: He says that when the plaster was taken off the ankle was still very much swollen; what do you say as to that? *A.*: No, it was not. It was swollen a little bit, certainly.

Q.: During the whole of the time that the plaintiff was in the plaster was there any complaint made by him that reached you or to you in connexion with the ankle? *A.*: Not to my knowledge.

Q.: What instructions did you give him when you took the foot out of the plaster? *A.*: He was to get up on crutches and take things very quietly for two or three days.

Q.: How did he come to leave the hospital? *A.*: On two occasions, one three or four days before he left, he asked could he leave. He said he wanted to go to Sydney on urgent business. I told him that I thought he ought to stay a little while longer, that the leg required a little more rest. He then asked me again in a couple of days' time could he go. I explained to him that if he thought he should go, he could go, but I thought he should stay a little while longer.

Q.: He said that he saw you every day; he told you that he could not walk at all. Could he walk at all? *A.*: No, he could not walk.

Q.: Then he says on the 7th October, which was the day he left, you were trying to get him to walk without any crutches at all. Is that correct? *A.*: No.

Q.: He said he tried and that he said: "It is impossible, Doctor, I cannot walk at all." Did you ever try to get him to walk at all without crutches or without support? *A.*: No.

Q.: And that you told him he only needed exercise? *A.*: I told him to take things very quietly.

Q.: Is it a fact that you told him that he only needed exercise? *A.*: No.

Q.: That he then said to you: "Doctor, can you do any more for me?" and that you said: "No." He said: "Can I go?" and you said: "You can go if you like. You want to get off the crutches as soon as you can." Is there any truth in that? *A.*: I have no recollection of it whatsoever.

Q.: When did you see him next after that? *A.*: After he came back from Sydney in, I should think, about three weeks' time. He came back and he was in plaster again. He came to my surgery. He said that while he was in Sydney he thought perhaps he had been getting about a bit too much and his leg began to swell. While he was at home his father saw the ankle and was a little bit worried and as Dr. Stevenson was in the house attending to his sister he asked him to have a look at it. Dr. Stevenson did so and sent him to the Mater Misericordiæ Hospital at North Sydney to be X rayed and he then put him back in plaster and he sent up a message to tell me particularly that there was a fracture of the fibula and tibia in good position.

Q.: Did he show you anything on that occasion? *A.*: Yes, he showed me the Mater Misericordiæ Hospital rays. I had a look at them and I asked him where the fracture of the tibia was, as he seemed to be in doubt; he explained to me and I explained to him that that was the area where it had been chipped off.

Q.: Had you any doubt about the fracture of the tibia? *A.*: Not the slightest.

Q.: He suggests that you never knew there was a fracture of the tibia at all? *A.*: That is wrong.

Q.: And that you had never treated a fracture of the tibia? *A.*: That is quite wrong.

Q.: We have been told it is apparent to anyone who can read an X ray at all that there was a fracture of the tibia. Had you any difficulty in reading it? *A.*: No.

Q.: Could you find any difference between the Mater Misericordiæ Hospital ray and your X ray? *A.*: No.

Q.: Did you tell him so? *A.*: Yes.

Q.: On that occasion he says that he came in on his way home, he called in and saw you and the first thing he said to you was: "Well, Doctor, my ankle was broken"? *A.*: No, I have no recollection of that at all.

Q.: You see the suggestion, that you have not known it was broken and he was telling you that it was broken? Then he says that you asked him if he had the X ray and he said yes. Then you asked him would he mind if you had a look at it. He handed you the X ray. You studied it and asked him where he thought the fracture was? *A.*: I asked him where he thought the trouble was, and explained to him that that was the chip that he had off the posterior aspect of the tibia.

Q.: Then he said that he showed you and that you said that would not cause any trouble at all? *A.*: I have not got any recollection of that.

Q.: Then he says that you and he had a lot of difference over the X ray. Had you any difference with him? *A.*: No, not as far as I remember.

Q.: And that he suggested you should have it sent down to an expert? *A.*: Yes, he made that suggestion and I agreed with it.

Q.: Then he went on to say that he would pay the cost of the reading if you would name a man, and that you said you did not know any. Is that true? *A.*: No.

Q.: Did he say what Dr. Stevenson told him? *A.*: He said that Dr. Stevenson had asked him to get me to take the plaster off in three weeks' time. The next time I saw him was in three weeks' time. He came back and I took the plaster off.

Q.: What condition was the ankle in then? *A.*: There was practically no swelling in the ankle itself. As a matter of fact it was rather on the thin side from disuse and the ankle was naturally stiff.

Q.: Did you see the plaintiff at all again after that—I mean to say professionally? *A.*: He came in some time in January—early in January or late in December, 1928, and he was still a little bit worried about his foot and he was going to Sydney and he said he would like to see Dr. Royle and I gave him a letter to Dr. Royle.

Q.: After he had gone to Dr. Royle, did you see him again? *A.*: He came back, I did not know how long after, it may have been about a week, and he said that Dr. Royle had said there was a little trouble there.

Q.: Was that the last time you saw him? *A.*: Yes.

Q.: In your experience does a patient who has suffered such an injury as this ever get complete restoration? *A.*: No.

Q.: If you had a case with very little displacement or practically no displacement, might that in the result turn out as bad or worse than one with a big displacement? *A.*: It might turn out very much worse.

HUGH RAYMOND GUY POATE, sworn, examined, deposed:

To Mr. Feez: I am a legally qualified medical practitioner carrying on my practice in Macquarie Street. In a fracture of this sort the usual treatment is first of all to examine the patient and if you can bring the foot into proper alignment you do so immediately. If there is any difficulty you administer an anæsthetic and set it under the anæsthetic. As far as splinting is concerned, the usual splinting adopted is a box-splint. Of course a few of us use plaster immediately, but the box-splint is the usual form of treatment. The usual thing is to put it into plaster. I have never seen one fracture of this sort restored to normality. The best result you could get would be a 10% to 15% impairment of the functioning of the joint. That would be regarded as a good result—a permanent disability to that extent.

Q.: Supposing you found that the foot could be brought up to a right angle although the swelling existed, what would that indicate to you? *A.*: If you could bring that foot in the presence of swelling up to a right angle, that would necessarily indicate that the position of the bony fragments must be relatively good, so much so that you would not worry about any further manipulation under an anæsthetic. When I say relatively good I mean satisfactory, and that being the case, of course you would not give an anæsthetic and you would not in any way try to manipulate that leg. That is just the position you want to get it into. I should say that ninety-nine out of one hundred practitioners put the leg into a box-splint. The box-splint is the usual practice in the hospitals. Then after the swelling has gone the leg is put into plaster. If I find after an accident of this sort that the patient's foot can be dorsiflexed to a right angle, it is a perfectly safe thing to put it up in a box-splint at that right angle. You must maintain that right angle. I would not find any fault with a doctor having put him into a box-splint, after eight days put him into plaster and after a fortnight put him on crutches. That is the recognized form of treatment and I should say the times are about the average. I look at Exhibit "A" to see if there is any backward displacement in that X ray. Of course you would really want to compare this with the normal foot, but if there is any backward displacement it is very slight. Judging from that X ray and from my experience of them, I would say there is nothing there to worry about.

Q.: Even supposing there was some slight displacement of the astragalus, what do you say with regard to the result? *A.*: I should say, in view of the type of fracture he has had, that it is a pretty fair result.

SIR ALEXANDER MACCORMICK, sworn, examined, deposed:

To Mr. Feez: I am a legally qualified medical practitioner and have been practising in Sydney for some years. I examined the plaintiff, Mr. Mackellar on the second of this month in conjunction with Dr. Corlette. I got his history, when the accident happened and got an account of the treatment. He had an X ray photograph and I examined the limb. I got him to take off both boots and both socks and I examined the two limbs together.

Q.: What did you find in appearance with regard to the two feet? *A.*: There was very little difference between them. I manipulated the foot.

Q.: Will you explain exactly how you did manipulate it? *A.*: I tried to move it to see how much he could bend it upwards and downwards. I tried it sideways, too. I found with regard to that that he could not point his toes as much as he normally should. There was very little difference in the movement upwards. The dorsiflexion was very little different. I tried to get him to stand on his toes. He was able to do it to some extent. I got him to walk and he walked very well. I noticed a difference in his walking from the normal walking; he seemed a little stiff about the ankle. He had a fracture at the joint and you are not surprised to see a certain amount of disability like that after a fracture of that sort.

Q.: Was there any wasting in his leg or anything of that sort? *A.*: No.

Q.: You heard the method of treatment used in his case—eight days in a box-splint, with the foot at right angles and then afterwards a fortnight in plaster. What do you say as to that? Is that good treatment? *A.*: Yes. Of course the person who saw the patient in the first instance could judge best.

Q.: You have had a great deal of hospital experience? *A.*: Yes.

Q.: Is that the usual treatment? *A.*: Yes.

Q.: What do you say about the result that he has got after such an injury? *A.*: I think the result is very good.

Q.: What do you say is the present cause of his disability? *A.*: The fracture extending into his joint.

Q.: It has been suggested that this man is going to be crippled for life; what do you say as to that? *A.*: I do not think he will be.

CYRIL ERNEST CORLETTE, sworn, examined, deposed:

To Mr. Feez: I am a legally qualified medical practitioner practising in Sydney. I know the plaintiff, Mr. Mackellar. On examination there is some lateral flexion. X ray shows that posterior portion of the tibia was flaked off and some backward displacement of the talus and foot occurred and the piece of tibia has remained at a slightly higher level than the front. There is practically no difference in dorsiflexion now between the two ankles. That is the whole of my note.

Q.: Would you look at the X ray, Exhibit "A"? I want to ask you about the suggested backward displacement of the talus. Can you see any backward displacement of the talus there? *A.*: From the position that foot is in it is extremely difficult to say. It is a very fine point there to say whether there is displacement or not. Of course I know that the bone here projects a little further back than on the other side probably. I know that from looking at the bone. Although I would not without that knowledge say that the foot was displaced backwards, it is displaced back just about as much as that bit of bone is displaced and that is very slight indeed. I reckon that is a very good position.

Q.: You see the position of that foot; what do you say with regard to that? *A.*: I say anybody getting a fracture like that and a result like that is very much to be congratulated. I speak from a large experience. I would say he got off very lightly indeed.

Q.: After this accident happened we are told that the patient was put into a box-splint for eight days with his foot at a right angle, the foot firmly braced to the upright at a right angle, side splints as well, kept in that box splint for eight days and then put into plaster for a fortnight and then he was kept on crutches for some eleven days before he left the hospital. What do you say to that—is that good treatment? *A.*: It is not what I would do myself, but it is quite orthodox.

Q.: Is that the usual surgical treatment? *A.*: Yes, quite usual.

Dr. A. J. Aspinall, Dr. J. L. McKelvey, Dr. George Bell, Dr. H. S. Stacy and Dr. Stratford Sheldon gave evidence similar to that of Dr. Poate, Sir Alexander MacCormick and Dr. Corlette. Dr. A. S. Evans also gave evidence.

MARTHA SMELSON, sworn, examined, deposed:

To Mr. Feez: I am the matron of Gulgong Hospital and I have been in that position for nearly twelve years. I remember the plaintiff first coming to the hospital. When he arrived he was put to bed and Dr. Allport came down almost immediately after. He took him up to his surgery and had an X ray taken. Then Mr. Mackellar returned to the hospital and he was put back to bed. Then about twenty minutes after the doctor came down and put the leg up in splints. The doctor explained to him that the fibula was broken and there was a chip off the tibia. After the plaintiff had been put in the box-splint the doctor asked him if he felt more comfortable and he said he did. He was suffering pain, more from the back. Whatever it was that fell on him, fell on the back, and that was what

caused him pain. That was what Mr. Mackellar told me. Generally after that first day he complained of pain in the back. He did not complain of very much pain in his ankle. Occasionally it was sore, but no more than usual. He left the hospital in October. The doctor did not wish him to leave when he did. He asked the doctor could he leave, and the doctor thought it would be better if he stayed a few days longer to get the foot stronger. However, he wished to go, so the doctor let him.

The jury returned a verdict for the plaintiff for £925.

His Honour said: "I may say that, if it is any satisfaction to the doctor in regard to his professional reputation, I would have come to a different conclusion if I were trying the case."

Correspondence.

THE WAR AND SIR NEVILLE HOWSE'S PART THEREIN.

SIR: "*De mortuis, nil nisi bonum*" is a worthy sentiment, only to be amended when far-reaching issues are involved and justice or hope make a contrary demand. Under these conditions I give my own experience at the war, from first to last, in many places and positions, as written from day to day in my diary, the "*ipsissima verba*" of which are already in the hands of authorities and deposited in the Mitchell Library, corroborated, as it has been in its main facts by the Official Historian, Colonel Butler, in Volume I of his history.

Taking events in their sequence.

I. As shown by Butler, the evolution of our A.A.M.C. was poor and the mobilization of our A.I.F. unsatisfactory, both administratively and preventively. In my judgement both would have been much better developed if either Williams or Howse had remained behind and given Australia the benefit of their skill and experience.

II. Both shared medical responsibility on the first convoy. Butler's account of medical happenings can scarcely be said to be to the credit of either.

III. At Mena Howse was in medical charge for some three or four strenuous months. As matters of fact, there were amounts and severity of sickness amazing amongst picked troops in a world-wide sanatorium during its season also: no provision for isolation, invaliding or convalescence, no facilities for dentistry or massage, latrines open, fly breeding unimpeded, and numerous unnecessary operations. On our arrival (2 A.G.H.) we found the R.M.O.'s complaining, the men underfed and overtrained, the camp improperly placed and our hospital tents being pitched in Nile-washed sand. Howse's greeting was: "Whatever do you men want with an X ray outfit." The amount of disease was astounding and ever increasing. There were few signs of catering for the "humanities," and no sports, but full, unsupervised leave to Cairo, then more than ever a hotbed of vice and drugging. Whilst doing "our jobs" remedially we were shocked, investigated and reported to all authorities. After unnecessary delay, the D.M.S. Egypt nominated an Australian Committee of Enquiry, of which, as senior, I was President. This met after the troops had left, and months later its report, condemnatory all round, was signed by two only out of its five members, myself and Captain Summons. In the course of time the camp itself was condemned. Worst of all, Howse had the reputation of "knifing" anyone criticizing or opposing, though "the emotions rule the intellect" and may upset even the best laid schemes. And so it proved, later on.

IV. At Anzac Howse was mainly responsible medically from landing to evacuation. Butler's account is one terrible indictment, ending in "medical failure." True, the local obstacles were stupendous and the control both multiple and confusing. The "*Quicunque Vult*" of the Athanasian creed was translated into "Whosoever will be promoted must believe the almost unbelievable." Under such trying circumstances all agree that Howse showed great administrative activity, energy and endurance; but apparently he lacked foresight, insight and initiative and was too subservient to the Book of Regulations. At any rate, outsiders in Egypt, with little responsibility and less powers, felt it their duty to intervene. They sought to have transports manned for their own men by their own staff and to have Australian hospitals established in Alexandria. Neither was ever done. Nor did our general hospitals ever receive anything like their due proportion of bad cases, whilst many of the so-called "septic" proved to be "paratyphoid." We found also that the early diarrhœas were due to "improper food," not "infection." And our diagnosis was corroborated by the M.E.F. Advisory Committee, on which no place was ever found for even one Australian representative. Later on our hospitals in Egypt were crowded with diarrhœal and dysenteric cases, sent for treatment and convalescence to our torrid climate (in our sand egg albumen coagulated), a procedure for recommending which I would have "plucked" a student in Melbourne. And when the abdominal cases had invaded some two-thirds of the men at Anzac, it was on our insistence that "suitable foods" were forwarded and "suitable red cross comforts"—even Australia's "Xmas gifts"—rushed to the front. Further, before the days of cold and blizzards we sought to secure a "winter ration," but were refused, apparently by the D.M.S. Egypt. In so far as he had the power and the foresight, calamities such as these, wherever arising from neglect or otherwise preventible, must be laid to the charge of Howse.

V. Meantime, in Egypt matters medical had become unbearable; junior officers, both R.A.M.C. and A.A.M.C., were doing their very best within the regulations. But many of the senior R.A.M.C., resurrected under the exigencies of Kitchener's Army, had had a black mark placed against their names, "never to be employed again"; yet such "ruled the roost." Our 2 A.G.H. was described as "Woolloomooloo running Macquarie Street and Collins Street," whilst 1 A.G.H. had an able O.C., urged on by an ambitious "all round expert." Our O.C., though S.M.O., A.I.F., was always behind the times, whilst both the other leaders were ordered back to Australia and their extraordinary "extensions" condemned.

Thus "Australia must have its own D.M.S., A.I.F."—one who at least would carry weight, had experience and was energetic. The only man available was recognized to be Howse. After due consultation and many difficulties he was appointed and gazetted. Naturally he selected his staff temperamentally—two friendly consultants, three "regulational" A.D.M.S. and a splendid first convoy Matron-in-Chief. Not unnaturally, the results were less satisfactory than was hoped and not lasting. The D.M.S. himself exercised little external power. And before all his staff, except the junior A.D.M.S. (also Staff Officer to the D.M.S. Egypt) left for the Western Front, our hospitals were again reporting to D.M.S. Egypt through one of his A.D.M.S., whilst no new consultants were appointed to take charge of what was left in Egypt, though suitable men were available to his knowledge. Hence, neither efficiency nor success warrant the eulogium, "one of the best Directors of Medical Services at the war." In my judgement, very far from it.

VI. As regards subsequent events—in Egypt, England and France—I defer comment until the appearance of Butler's next volume, in which he will, I hope, not only detail actual happenings, but also give official views *re* praise or blame and tell Australia what, in his opinion, have been the main lessons of the war.

Meantime, it seems to me that the grim old Sphinx who, with wistful gaze, had watched his rising God flood Egypt and the Nile for thousands of years, must surely put on a look of surprise whenever he thought of the new nation that entered from the south, one of wonder when it proved itself "the bravest thing God ever made," and one of regret for the grave misfortunes that shrouded its dawn.

Yours, etc.,

J. W. SPRINGTHORPE,
Lieutenant-Colonel,
Australian Army Medical Corps.

Melbourne.
December 17, 1930.

Obituary.

CHARLES HENRY MOLLOY.

WE regret to announce the death of Dr. Charles Henry Molloy which occurred at Meeniyan, Victoria, on January 21, 1931.

THOMAS O'LOGHLEN REYNOLDS.

WE regret to announce the death of Dr. Thomas O'Loghlen Reynolds, which occurred at Camberwell, Victoria, on January 23, 1931.

Books Received.

PHYSICAL DIAGNOSIS, by W. P. Elmer, B.S., M.D., and W. D. Rose, M.D.; 1930. St. Louis: The C. V. Mosby Company; Melbourne: W. Ramsay. Royal 8vo., pp. 903, with 337 illustrations. Price: 50s. net.

PHYSIOLOGICAL CHEMISTRY, A TEXT-BOOK AND MANUAL FOR STUDENTS, by A. P. Mathews, Ph.D.; Fifth Edition; 1930. London: Baillière, Tindall and Cox. Royal 8vo., pp. 1242, with 109 illustrations. Price: 31s. 6d. net.

DIET AND CARE OF THE SURGICAL CASE, by R. H. Boyd, M.B., Ch.B., F.R.C.S., with an introduction by C. C. Choyce, C.M.G., C.B.E., F.R.C.S.; 1930. London: H. K. Lewis and Company Limited. Crown 8vo., pp. 116. Price: 5s. net.

RECENT ADVANCES IN THE STUDY OF RHEUMATISM, by F. J. Poynton, M.D., F.R.C.P., and B. Schlesinger, M.A., M.D., M.R.C.P.; 1931. London: J. and A. Churchill. Demy 8vo., pp. 321, with 25 Illustrations. Price: 12s. 6d. net.

RECENT ADVANCES IN ENTOMOLOGY, by A. D. Imms, M.A., D.Sc., F.R.S.; 1931. London: J. and A. Churchill. Demy 8vo., pp. 382, with 84 illustrations. Price: 12s. 6d. net.

THERAPEUTIC USES OF INFRA-RED RAYS, by W. A. Troup, M.C., M.B., Ch.B., with Foreword by Sir William Willcox; 1930. London: The Actinic Press, Limited. Demy 8vo., pp. 57, with illustrations. Price: 5s. 6d. net.

OPERATIVE GYNECOLOGY, by H. S. Crossen, M.D., F.A.C.S., and R. J. Crossen, M.D.; Fourth Edition; 1930. St. Louis: The C. V. Mosby Company; Melbourne: W. Ramsay. Crown 4to., pp. 1078, with 1246 illustrations and two colour plates. Price: 75s. net.

PRACTICAL TREATISE ON DISEASES OF THE DIGESTIVE SYSTEM, by L. Winfield Kohn, M.D., F.A.C.P., Volumes I and II; 1930. Philadelphia: F. A. Davis Company. Royal 8vo., pp. 1156, illustrated with 542 engravings, including 7 full-page coloured plates. Price: $12.00 net.

Medical Appointments.

Dr. J. B. Cole, Dr. A. A. Hinchley and Dr. W. L. Jack have been reappointed as Resident Medical Officers at the Adelaide Hospital, South Australia.

* * *

Dr. R. G. C. de Crespigny, Dr. S. J. Douglas, Dr. A. J. Hakendorf, Dr. W. W. Jolly, Dr. R. Krantz, Dr. V. F. B. Lennon, Dr. B. G. Maegraith, Dr. J. D. Rice, Dr. F. J. Ryan and Dr. R. J. Wheeler have been appointed Resident Medical Officers at the Adelaide Hospital, South Australia.

Medical Appointments Vacant, etc.

FOR announcements of medical appointments vacant, assistants, *locum tenentes*, sought, etc., see "Advertiser," pages xii and xiii.

COMMONWEALTH DEPARTMENT OF HEALTH: Medical Officer.

MASONIC HOSPITAL, ASHFIELD, SYDNEY, NEW SOUTH WALES: Four Honorary Physicians, Four Honorary Surgeons, Resident Medical Officer.

ROYAL HOSPITAL FOR WOMEN, SYDNEY, NEW SOUTH WALES: Resident Medical Officer, Junior Resident Medical Officer.

Medical Appointments: Important Notice.

MEDICAL practitioners are requested not to apply for any appointment referred to in the following table, without having first communicated with the Honorary Secretary of the Branch named in the first column, or with the Medical Secretary of the British Medical Association, Tavistock Square, London, W.C.1

BRANCH.	APPOINTMENTS.
NEW SOUTH WALES: Honorary Secretary, 135, Macquarie Street, Sydney.	Australian Natives' Association. Ashfield and District United Friendly Societies' Dispensary. Balmain United Friendly Societies' Dispensary. Friendly Society Lodges at Casino. Leichhardt and Petersham United Friendly Societies' Dispensary. Manchester Unity Medical and Dispensing Institute, Oxford Street, Sydney. North Sydney Friendly Societies' Dispensary Limited. People's Prudential Assurance Company, Limited. Phœnix Mutual Provident Society.
VICTORIAN: Honorary Secretary, Medical Society Hall, East Melbourne.	All Institutes or Medical Dispensaries. Australian Prudential Association Proprietary, Limited. Mutual National Provident Club. National Provident Association. Hospital or other appointments outside Victoria.
QUEENSLAND: Honorary Secretary, B.M.A. Building, Adelaide Street, Brisbane.	Members desiring to accept appointment in ANY COUNTRY HOSPITAL, are advised to submit a copy of their agreement to the Council before signing, in their own interests. Brisbane Associated Friendly Societies' Medical Institute. Mount Isa Hospital. Mount Isa Mines.
SOUTH AUSTRALIAN: Secretary, 207, North Terrace, Adelaide.	All Lodge Appointments in South Australia. All Contract Practice Appointments in South Australia.
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